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THE PROGRESS OF CHEMICAL THEORY: ITS HELPS AND HINDRANCES.

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[*Introduction to the chemical course. A lecture delivered before the Franklin Institute, November 21, 1890.*]

[*Continued from p. 252.*]

Hofmann (1660–1742), the first to distinguish between magnesia and alumina, asked whether instead of the above explanation of combustion it was not the case that a metal received an "acid" when burned, which "acid" was re-absorbed when the metal was reduced. Besides him many others had noted the increase of weight in combustion, and Boerhave (1668–1738) had doubted the explanation above referred to.

The offence of which as seekers after truth the phlogistonites were guilty was that of calling into existence a substance which was diametrically different in its properties

from any then known, and making this imaginary substance and its purely imaginary properties the basis of a theory.

Isaac Newton (1642–1727) had established, by his matchless investigations, commencing 1666, the nature and properties of matter, one of which, gravitation, was common to all bodies, and there was no excuse for an hypothesis after that date, which should deliberately rob matter of its one all-pervading attribute, unless some discovery had been made which seemed to support it.

If it be said that the observations on the combustion of bodies seemed themselves to furnish this proof, it must be admitted that for such an overthrow of all that had been patiently built up, some independent testimony unconnected with the then obscure phenomena of combustion ought to have been sought. This was an unpardonable deflection from the line of calm and dispassionate inquiry, and deserves to be held up for all time to the condemnation of scientific men as a warning, and its fate as an example; the more so because as time went on and the array of obstacles to the acceptation of this theory increased, its supporters were obliged to set up one after another distinct hypotheses to support the first untenable one, until the discussion partook more of the nature of those verbal juggles popular among the schoolmen of the middle ages than the efforts of students of nature to arrive at a knowledge of her laws.

Marggraf (1709–1783) introduced into chemistry the study of reactions in the wet way, and thus laid the foundation of analytical chemistry. He recognized soda as an alkali, and magnesia and alumina as peculiar earths. Against the opinions of his predecessors he held that ammonia was not produced in the distillation of wood, etc., by the union of its constituents, but that it pre-existed in the wood.

J. Black, of Scotland, by his investigation of the alkalies, dealt the first serious blow to the phlogistic theory. He found that magnesia could be changed to "mild" from having been strongly alkaline by exposure to air, or by contact with "mild" alkalies. In other words, that it became carbonate of magnesia by exposure to the carbonic acid of the air or by treatment with the carbonates of the alkalies.

Furthermore, he showed that what it lost or gained in these changes was a gas like air, which separated from it under treatment by acids with effervescence, whereas in its caustic condition acids dissolved it without effervescence. He also, with singular astuteness, established for his theory of combustion (which was to finally take the place of the phlogistic theory) the principle of latent heat. The immediate result of Black's discoveries was the rise of pneumatic chemistry or the chemistry of the gases.

Joseph Priestley (1733–1804) followed with more success than any other this line of investigation. He worked sporadically and without system, but with wonderful penetration discovered many things that had escaped the attention of better chemists. He himself called his achievements "sportsman's luck." He found the nitrogen left after combustion in air, and determined its proportion by volume. He regarded it as charged with phlogiston.

In 1774, he discovered oxygen in the oxide of mercury, and recognizing it as the supporter of combustion in the air, made it the measure of the destruction or devitalization of the air. He discovered besides oxygen and nitrogen, the reduction of the calxes by hydrogen. He also observed that by the passage of the electric spark through confined air in contact with moistened litmus, a new acid was produced which colored the latter red. He furnished more material than any other chemist of the day for the destruction of the false phlogistic theory, but he was not only incapable of correctly reasoning on the facts which he had brought to light, but to the day of his death he maintained that his pivotal discovery; the corner-stone of the chemistry of to-day (as it was made by Lavoisier) was nothing but the production of dephlogisticated air.

It was a touching thought for those of us who, in 1874, assembled at his grave in Northumberland, Pa., to do honor to his memory as a great discoverer and a devoted friend of our young States when we threw off the English yoke, and to celebrate the centennial anniversary of the foundation of modern chemistry, in his isolation of oxygen; that this great mind so potent in dis-

covery in the science which he adorned, rejected the obvious fruits of that discovery, and insisted on rejecting the honor which should justly have been his.

H. Cavendish (1731–1810) was a more rigid investigator, who, having discovered hydrogen by the action of sulphuric or hydrochloric acid on iron or zinc, proceeded to examine exhaustively the properties of this new gas. He noted that like weights of metal gave him the same volumes, but that different weights of metals gave him different volumes of the gas. He concluded that hydrogen was either phlogiston or a combination of phlogiston with water, and that dephlogisticated air (oxygen) when combined with phlogiston (hydrogen) produces water.

He discovered the solubility of carbonates of the alkaline earths in excess of carbonic acid water, and discovered nitric acid. He was thoroughly saturated with the phlogistic theory and all the useful deductions which his careful methods would have given him were thrown away by the distortions of the obvious bearing of his experiments which the phlogistic theory necessitated. When Lavoisier had triumphantly overthrown this dragon, Cavendish abandoned chemistry; simply remarking that it was hard to determine which of the rival theories was true.

Passing over the labors of the phlogistonists: Scheele, who discovered chlorine and fluo-silicic acid, and who believed light and heat to be fire and air; the first with more and the second with less phlogiston; and Bergmann, who systematized wet analysis, proved the existence of carbonic acid in the air, and introduced dry qualitative blowpipe analysis; we come to the first of a number of great figures in the history of modern chemical theory whose mission it was to open the door of the exact science of to-day and to indicate the route of those who followed him.

Lavoisier (1743–1794). His first chemical work was the experimental proof that water did not become an earth by boiling, but that the residue observed by boiling in a glass vessel was derived from the glass. His own discoveries are meagre in comparison with those of many of his con-

temporaries; but he greedily absorbed all that was discovered by others and changed it from crude and disconnected facts into an harmonious and consistent system.

All of his contributions to chemical science were of this character, even that great conception of the difference between the least distinguishable part of a certain kind of matter, and the least part which can take part in chemical reactions—the germ of the future distinction of the then unknown atom and molecule.

The last entrenchment of the phlogistonites was in the observed action of acids on certain metals whereby hydrogen (phlogiston) was produced. "If," they said, "a metal be not a combination of a calx with phlogiston, whence comes the phlogiston produced by the experiment?" The obvious answer was that it came from the acid, but what part? The acid was thought to unite with the metal and dephlogisticate it. Priestley and Cavendish had shown that hydrogen (phlogiston) and dephlogisticated air combined and in great part disappear. What became of them was not satisfactorily settled for a quite surprisingly long time. The fact that a little moisture was observed in the apparatus was probably ascribed to the sudden expansion and contraction of volume of the gases; since these matters and the hygroscopicity of the air were only beginning to be understood. When, therefore, Cavendish announced his discovery of the composition of water, Lavoisier applied it to the theory of combustion with such telling effect that it once and for all overthrew and destroyed phlogiston.

But though this was a glorious service of Lavoisier, it was far from all that chemistry owes to him. The new conceptions required a new language to express them, and Lavoisier with Guyton de Morveau established a system and a nomenclature so perfect as to form the frame work enclosing within itself all systems which followed.

A series of definitions and rules for naming new combinations as well as a partial list of elements were parts of this system.

Lavoisier came back to Robert Boyle's definition of an element as a body which cannot be decomposed into simpler

ones. The table of these elements, published by the French colleagues, contains also heat and light, but this was not in conformity with the opinion of Lavoisier, who was far too careful a student of nature to commit himself to any such gratuitous assumption; but rather more probably a concession made to the defeated phlogistonists and a means of avoiding the necessity of explaining that about which the views of scientific men were very conflicting, and nothing was certainly known.

One generalization Lavoisier allowed himself, and that was that all acids contain oxygen (hence the name he gave to that element). It is curious how fate punish s such generalizations whenever they are made in the infancy of our knowledge on any subject. This generalization reflects great credit upon its proposer, and shows a rapid and profound insight into many facts, but it was not many years afterwards pronounced, and is now generally considered, a fallacy, and yet if we interpret it to mean the exhibition of acid characters such as the reddening of litmus and the effect upon the senses, Lavoisier was not entirely wrong; since these characters are inseparable from the presence of water which contains oxygen.

The long chemical war against phlogiston had been fought and won, and the thought of experimenters was turning in a new direction which was to institute a new war lasting only a little less long than the last, but the difference between the two cases was that whereas the phlogistic theory hung like a pall over the whole science, obscuring during its continuance the entire field; in this case the question in dispute was as to the ultimate constituents of matter and none of the many views entertained on these questions interfered with the classification and assimilation of the myriads of facts which experiment and research were eliciting. This war, therefore, while it will serve to illustrate that the most eminent chemists share with the rest of the world the weaknesses of our common humanity, did not materially retard the progress of the theory of chemistry.

Proust (1755–1826) announced the unchangeable propor-

tions by weight in which substances combine together; and that if they combine in more than one proportion it is by leaps and not gradually as the water of the ocean becomes little by little more charged with salts brought down to it by the rivers. This was a great and pregnant discovery which at once led the way to the new field of battle, but the strangest thing about this announcement is that it was vehemently attacked by Berthollet (like Proust, a native of France) in a proposition which a little later seemed nothing but a stupid blunder or obstinate opposition, and yet in Berthollet's contention lay a precious truth only recently recognized and placed in its proper place.

Briefly the skirmish between these two men was this: Proust discovered that the relative proportion to each other by weight of carbonic acid and copper in carbonate of copper was constant, no matter in how great excess one or the other of these bodies was present, the weight of the carbonate of copper was the same and the weight of each constitutent in it was invariable. For instance, substituting the accurate weights which better methods and apparatus have enabled chemists to obtain for the inaccurate approximations then made, in 123·4 grams of carbonate of copper, (there were always 63·4 grams of copper and 60 grams of carbonic acid. It made no difference whether these weights of the two elements respectively, or whether two or three times as much of one with the above weight of the other were made to combine: the result was always that 123·4 grams of the compound were found and the excess of either element remained uncombined.

With tin and iron there were two proportions by weight in which each of these elements combined with oxygen, but there were no insensible passages from one to the other. Thus there was a compound of oxygen and tin in which 119 weight-units of the latter combined with 16 of the former; and there was another in which 119 tin combined with 32 of oxygen but there was none in which the 119 weight-units of tin combined with any number of weight-units of oxygen between sixteen and thirty-one.

Berthollet (1748–1822) maintained, on the contrary, that if different masses of two elements are brought together, there will be found in the compound more of that constituent which was in greater quantity before the union. On account of the high position which Berthollet held in the chemical world this view received respectful, though silent attention, for few of the masters of the science were won over by it; because Richter, Klaproth, Vauquelin, and Wenzel had placed the constancy of acid and base in a compound of the two, beyond all question. Proust, however, took up the gauntlet and followed each separate publication of Berthollet by a refutation based upon careful experiment. This lasted for eight years, or from 1799 to 1807, and was settled apparently forever when Proust, by repeating some of Berthollet's own experiments on the successive stages of oxidation showed that his opponent had mistaken a percentage of water for a percentage of oxygen.

But Berthollet's main idea that the mass and the affinity were inseparable factors in the formation of a compound, after having been crushed to earth was to rise again in more recent times by the labors of his countryman of almost similar name, Berthelot and St. Gilles, and by Guldberg and Waage; but they showed, not that the proportion by weight of the compound, but that the rapidity of the reaction was affected by the masses of the constituents.

This dispute and the rise and fall of a theory was only a slight skirmish, which was preliminary to the general engagement. It had an admirable effect on the science, widened men's views, proved that the weapon of the future was to be carefully conducted experiment; and without doubt ripened the next great discovery which was then about to be announced.

John Dalton (1766–1844) was led to the happy thought of taking the data of the weights which Proust had announced as those in which tin, iron, oxygen, etc., combined, and reducing them to their simplest proportions. Proust had found that some arbitrary number of grams of tin (say, for example, 119) combined either with 16 or with 32 grams

of oxygen, and with no other weights. Dalton showed that the weights of oxygen in these two compounds were to each other as 1 to 2.

In the same way the different weights of sulphur which entered into combination with a given weight of iron were to each other as 1 to 2. And he found that this held for all cases where two constituents combined with each other in more than one proportion.

Thus, if the amount of hydrogen in olefiant gas or ethylene and marsh gas or methane are compared they are to each other as 1 to 2. By numerous examinations of this kind, in all of which he found this simple relation, he was led to formulate his atomic theory, some of the more important propositions of which may be thus condensed. (1) Every element consists of similar atoms of fixed weight; (2) Chemical combinations are made by the union of the atoms in the simplest proportions. The atomic weight of a compound is equal to the sum of the atomic weights of its constituents. He supposed all the atoms to be spherical and to be surrounded by heat spheres (!)

It should be mentioned in passing that Higgins had said in 1789 that chemical smallest particles were united to form compounds in simplest proportions, but as he never adduced any proof of this, the merit of the discovery belongs to Dalton by the law of possession already alluded to before, viz: that in natural science not only must a truth be announced, but some reason for it must be given.

The immediate result of his postulates was that Dalton set out to establish a scale of atomic weights for the elements. Among minor postulates of his which have not lived till our day, but which were natural enough at a time when there were no means of obtaining certainty as to the questions of the number of atoms entering into combination, etc., was this, that if only one proportion by weight of a combination between two elements were known, it must be supposed that the number of atoms entering into this combination was one from each element. If two were known, then that in which the least weight of one combined with the least weight of the other must be considered 1 to 1;

when with double this weight of the other the proportion must be 1 to 2, etc. In Dalton's time only one combination between oxygen and hydrogen was known, viz: water, and he assumed this to be composed of one atom of H to one atom of O. As H was and is yet the lightest element known he assumed its weight as one. By the imperfect methods then available, he determined the weight of O which combined with it to form water as 6·5 (in reality it is 7·98 if H = 1).

Ammonia, which was the only compound of H and N known to him, and in which he also assumed one atom of each element, gave him the number 5 for the atomic weight of N. By accurate methods it should be 4·66.) In the lowest compound of carbon and oxygen known, carbonous oxide, he found the atomic weight of carbon 5·4 calling oxygen 6·5 (the right figure is 6).

All his figures were wrong as we now believe because of his false assumption of the constitution of water (not to speak of his imperfect methods of analysis), yet the accuracy which he attained was surprising for his epoch and the invigorating effect on the science was as great as if all his numbers had been absolutely correct.

Humphry Davy (1778–1829), the great discoverer of the alkaline metals and earths, who first announced the elemental character of chlorine, and by his discovery of the halogen acids seemed to have overthrown Lavoisier's dictum regarding the invariable presence of O in all acids: Davy, the discoverer of the safety lamp for miners, first announced his belief that chemical affinity and electricity were the same force. This idea was erected by Berzelius later into the splendid structure which he called the electro-chemical theory. Neither Davy nor Wollaston believed that Dalton's experiments had succeeded in establishing the nature and characters of atoms, but contented themselves with Wollaston's theory of "equivalents," without seeking to define how much matter entered into combination.

Their theory was that the atomic weights of Dalton were merely a series of arbitrary numbers, showing the respective quantities of different elements which were equivalent to each other in combining each with a third.

Wollaston's name of "equivalents" took root later after the apparent failure of Berzelius' theory to account for all the facts, and was the shibboleth of a long period of timidity and vacillation in chemical theory, which marked the reaction of thought when it was feared that the allurements of a beautiful system and the powerful influence of a great authority had drawn the representatives of the science away from sure ground. This period of intellectual cowardice was very tantalizing and very confusing to those who pursued their studies during this period, but in the end it was an advantage to the science by letting the field lie fallow for a time, and making it thus the fitter to receive and develop the seed which finally was sown upon it.

In all cases where the development of a science has been rapid, it is found that the great minds are clustered together, and that the great discoveries occur in succession to supplement each other. It was stated that the discovery by Black originated pneumatic chemistry or the chemistry of the gases. In this field the discoveries of Cavendish, Priestley and Scheele were made, but with the wider view given by Lavoisier to the science, the study of the gases was abandoned for the study of other solid and liquid compounds. But Gay-Lussac (1778–1850) devoted himself to pneumatic chemistry and accomplished in it what supplemented the work of Dalton and prepared the way and assisted the researches of Berzelius.

In 1805, in conjunction with Alex. v. Humboldt, Gay-Lussac established the fact that exactly two volumes of H combine with one volume of O to form water.

He showed the simple relations of the volumes of combining gases to each other and to their compound: he showed the effects of temperature on gases, and how it must be considered in connection with the Boyle-Marriotte law of pressure. His conclusion was that "The specific gravities of gases are proportional to their atomic weights, or are simple multiples of them."

Avogadro, an Italian chemist and physicist, attracted by the discoveries of Gay-Lussac, had, in 1811, deduced from the Boyle-Marriotte law that in equal volumes of two gases at

the same pressure and temperature must be contained an equal number of physical particles. It was such a small step from these two beautiful generalizations to the conclusion that the smallest physical parts of elementary gases not being indissoluble must contain more than one atom, and that therefore here was proven the physical smallest parts, and the still smaller chemical smallest parts which are capable of entering into combination. But though Avogadro announced this conclusion in 1811, it was long years before it was taken up and embodied in the theory of the science. Gay-Lussac furthermore, by his work on iodine and cyanogen, laid the foundation of the "radicle;" as his experiments on the action of chlorine on oils did the same for the "substitution theory." He is also the inventor of the method of volume analysis or titrimetry.

J. J. Berzelius (1779–1848) offers a life history such as few have been seen since the beginning of the world. It seemed as if the tangled skeins of nature's most intricate clues were straightened and cleared in his hand as if by magic; and the marvel of the magnificent work which he left as a monument to himself, his country and the science to whose cause he was devoted, is that hardly a line of it needs to be erased, and the corrections of his constants are decimals representing the greater accuracy of apparatus at the present day.

In 1812 he created a new mineral system, in which the combination of the elements in multiple proportion was clearly indicated. His improved methods of analysis served to enable him in 1814 to show that also in organic chemistry this law prevailed. The atomic theory was made by him the guiding principle for the science. He explained the union of elements by the polarities peculiar to their atoms and his electro-chemical theory founded upon this hypothesis brought him to the dualistic view of the combination of matter.

The reason why so little of his work needs to be changed is that he based everything upon investigation and experiment. The results which he achieved here will remain, no matter what theory may be the final outcome of further

advance. He saw at once that Dalton's rule for determining the relative number of atoms in a compound was arbitrary and he pronounced it so. With the help of Gay-Lussac's discoveries of the gas volume relations and his own discovery of the oxygen law of relation between the acid and the base, he was enabled to draw correct conclusions as to at least 2,000 bodies which he had personally analysed. He considered the unit volume represented by the atom, and he deduced the constitution of bodies by weight and by volume (as, for instance, water) as we understand them to-day. It is only fair to observe that Berzelius himself, in spite of his strong belief in the power of the volume theory to assist the investigator to a knowledge of the true atomic relations of a chemical element, recognized its limitations and rejected altogether the efforts to apply it to bodies which could not be studied in the gaseous state.

This is only just to bear in mind, because an onslaught against the splendid structure which his skilful hands had erected was caused by a mistaken notion as to Berzelius' real views of the atom volumes. This onslaught caused the paralysis of the faculty of speculation for many years among chemists, converting one of the most enchanting and exciting fields of discovery into the mechanical record of dry facts, which it was contrary to the fashion of the day to seek to unite under any common cause. Had Berzelius' teachings been properly understood and heeded, there had been no occasion for this panic, and the last days of the grand old Pioneer would not have been embittered by the thought that the labor of his life, which was good work, was doomed to destruction.

The atom values which he had obtained in 1818 are given herewith.

	Berzelius.	*Correct.*
C,	12·12	12·
O,	16·0	16
S,	32·3	32·06
Pb,	416·0	206·95
Hg,	406·0	200·00
Cu,	129·0	63·4
Fe,	109·1	56·0

	Berzelius.	*Correct.*
Na, ...	93·5	23·05
Ka, ...	157·6	39·11
Ag, ...	433·7	107·92

The reason that the values he obtained for the metals were so much higher than ours now, was that he then doubted the occurrence of other oxides than MO, MO_2, MO_3, etc., M standing for the metal. Instead of FeO, Fe_2O_3, he wrote these compounds FeO_2, FeO_3, and consequently his percentage of iron was doubled. For similar reasons the metals K and Na received four times their normal weight since he regarded the compound which we know now as K_2O_4 as KO_3. Some years later he modified this position, admitting the existence of M_2O_3, and his table then conforms nearly to the present. In his further classic work of determining the atom weights, he was assisted by the beautiful discovery of Dulong and Petit in 1819, that the atoms of all elements have the same capacity for heat, or that the product of the specific heat into the atomic weight gives a constant quantity.

Mitscherlich, a student of Berzelius, discovered that compounds of different elements which were similar in the number of atoms, the equivalent of water, etc., were isomorphic in crystallization.

[*To be continued.*]

RIVETED JOINTS IN BOILER SHELLS.

By William Barnet Le Van.

[*Read at the stated meeting of the Institute, held November 19, 1890.*]

[*Concluded from p. 267.*]

Fig. 11 represents the spacing of rivets composed of steel plates three-eighths inch thick, averaging 58,000 pounds tensile strength on boiler fifty-four inches diameter, secured by iron rivets seven-eighths-inch diameter. Joints of these dimensions have been in constant use for the last fourteen years, carrying 100 pounds per square inch.

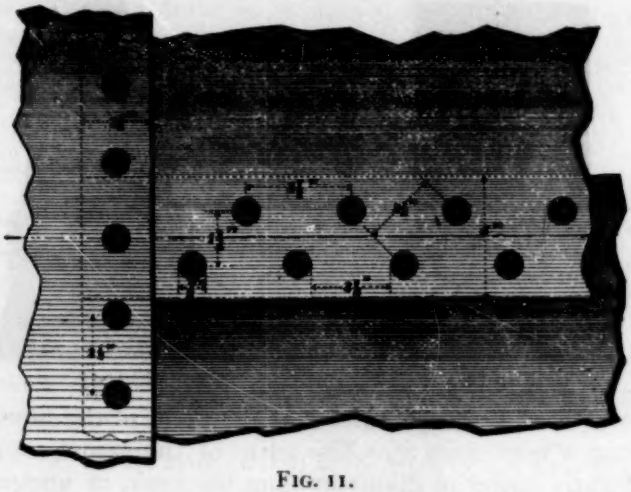

Fig. 11.

Punching rivet holes.—Of all tools that take part in the construction of boilers none are more important, or have more to do, than the machine for punching rivet holes.

That punching—or the forcible detrusion of a circular piece of metal to form a rivet hole—has a more or less injurious effect upon the metal plates surrounding the hole, is a fact well known and admitted by every engineer, and it has often been said that the rivet holes ought all to be drilled. But, unfortunately, at present writing, no drilling appli-

ances have not yet been placed on the market that can at all compare with punching apparatus in rapidity and cheapness of working. A first-class punching machine will make from forty to fifty holes per minute in a thick steel plate. Where is the drilling machine that will approach that with a single drill?

The most important matter in punching plates is the diameter of the opening in the bolster or die relatively to that of the punch; this difference exercises an important influence in respect not only of easy punching but also in its effect upon the plate punched. If we attempt to punch a perfectly cylindrical hole, the opening in the die block

FLAT *and* SPIRAL

PUNCHES.

FIG. 12. FIG. 13.

must be of the same diameter as the point of the punch, or, at least, a very close fit. The point of the punch ought to be slightly larger in diameter than the neck, or upper part, as shown in *Figs. 12* and *13*, so as to clear itself easily.

When the hole in the bolster or die block is of a larger diameter than the punch, the piece of metal thrust out is of larger diameter on the bottom side, and it comes out with an ease proportionate to the difference between the lower and upper diameters; or, in other words, it produces a taper hole in the plate, but allows the punching to be done with less consumption of power and, it is said, with less strain on the plate.

As to the difference which should exist between the diameter of the punch and die hole, this varies a little with the thickness of the plate punched, or should do so in all carefully executed work, for it is easy to understand that the die which might give a suitable taper in a three-fourths-inch plate would give too great a taper in a three-eighths-inch plate. There is no fixed rule; practical experience determines this in a rough-and-ready way—often a very rough way, indeed, for if a machine has to punch different thicknesses of plate for the same size of rivets, the workman will seldom take the trouble to change the die with every variation of thickness. The maker of punches and dies generally allows about three-sixty-fourths or 0·0468 of an inch clearance.

The following formula is also used by punch and die makers:

$$Clearance = D = d + 0{\cdot}2t$$

where

D = diameter of hole in die block;
d = diameter of cutting edge of punch;
t = thickness of plate in fractions of an inch;

that is to say, the diameter of the die hole equals diameter of punch plus two-tenths the thickness of the plate to be punched.

Example.—Given a plate $\frac{3}{8}$ or 0·375 of an inch thick, the diameter of the punch being $\frac{13}{16}$ or 0·8125 of an inch, then the diameter of the die hole will be as follows:

$$\text{Diameter of die hole} = 0{\cdot}8125 + 0{\cdot}375 \times 0{\cdot}2 = 0{\cdot}8875 \text{ inch diameter,}$$

or say $\frac{7}{8}$ or 0·875 inch diameter.

Punches are generally made flat on their cutting edge, as shown in *Fig. 12*. There are also punches made spiral on their cutting edge, as shown in *Fig. 13*. This punch instead of being flat, as in *Fig. 12*, is of a helical form, as shown in *Fig 13*, so as to have a gradual shearing action commencing at the centre and travelling round to the circumference. Its form may be explained by imagining the upper cutter of a

shearing machine being rolled upon itself so as to form a cylinder of which its long edge is the axis. The die being quite flat, it follows that the shearing action proceeds from the centre to the circumference, just as in a shearing machine, it travels from the deeper to the shallower end of the upper cutter. The latter is not recommended for use in metal of a thickness greater than the diameter of the punch, and is best adapted for thicknesses of metal two-thirds the diameter of the punch.

Fig. 14 shows positions of punch and attachments in the machine.

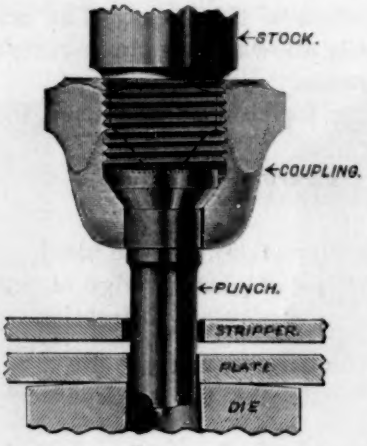

FIG. 14.

It is of the greatest importance that the punch should be kept sharp and the die in good order. If the punch is allowed to become dull, it will produce a fin on the edge of the rivet hole, which, if not removed, will cut into the rivet head and destroy the fillet by cutting into the head. When the punch is in good condition it will leave a sharp edge, which, if not removed, will also destroy the fillet under the head by cutting it away.

Punching possesses so many advantages over drilling as to render it extremely important that the operation should be reduced to a system so as to be as harmless as possible to the plate. In fact, no plate should be used in the construction of a boiler that does not improve with punching,

and further on I will show by the experiments made by Hoopes & Townsend, of Philadelphia, that good material is improved by punching; that is to say, with properly made punches and dies, by the upsetting around the punched hole, the value of the plate is increased instead of diminished, the flow of particles from the hole into the surrounding parts causing stiffening and strengthening.

Drilling rivet holes.—In the foregoing I have not referred to the drilling of rivet holes in place of punching. The great objection to drilling rivet holes is the expense, from the fact that it takes more time, and when drilled of full rivet size we are met with the difficulty of getting the rivet holes to correspond, as they are when punched of full-rivet diameter. When two plates are drilled in place together, the drill will produce a *burr* between the two plates—on account of their uneven surfaces—which prevents them being brought together, so as to be water and steam tight, unless the plates are afterwards separated and the burr removed, which of course adds greatly to the expense.

The difference in strength between boiler plates punched or drilled of full rivet size may be either greater or less than the difference in strength between unperforated plates of equal areas of fracture section. When the metal plates are very soft and ductile, the operation of punching does no appreciable injury. Prof. Thurston says he has sometimes found it actually productive of increased strength; the flow of particles from the rivet hole into the surrounding parts causing stiffening and strengthening. With most steel and hard iron plates the effect of punching is often to produce serious weakening and a tendency to crack, which in some cases has resulted seriously. With first-class steel or iron plates, punching is perfectly allowable, and the cost is twenty-five per cent. less than drilling; in fact, none but first-class metal plates should be used in the construction of steam boilers.

In the original punching machines the die was made much larger than the punch, and the result was a conical taper hole to receive the rivet. With the advanced state of the arts the punch and die are accurately fitted; that is to

say, the ordinary clearance for a rivet of (say) three-fourths inch diameter, the dies have about three-sixty-fourths of an inch, the punch being made of full rivet size, and the clearance allowed in the diameter of the die.

Take, for example, cold-punched nuts. Those made by Messrs. Hoopes & Townsend, Philadelphia, when taken as specimens of "commercial," as distinguished from merely experimental, punching, are of considerable interest in this connection, owing to the entire absence of the conical holes above-mentioned.

When the holes are punched by machines properly built, with the punch accurately fitted to the die, the effect is that the metal is made to flow around the punch and thus is made more dense and stronger. That some such action takes place seems probable, from the appearance of the holes in the Hoopes & Townsend nuts, which are straight and almost as smooth as though they were drilled.

Therefore, I repeat, that iron or steel that is not improved by proper punching machinery, is not of fit quality to enter into the construction of steam boilers.

STRENGTH OF PUNCHED AND DRILLED IRON BARS.

HOOPES & TOWNSEND.

Thickness of bar in inches.	*Thickness outside of hole in inches.*	*Punched bars broke in pounds.*	*Drilled bars broke in pounds.*
⅜ or 0·375	⅜ or 0·375	31,740	28,000
⅜ or 0·375	⅜ or 0·375	31,380	26,950
⅜ or 0·6 5	¼ or 0·25	18,820	18,000
⅝ or 0·625	¼ or 0·25	18,750	17,590
⅝ or 0·625	$\frac{3}{16}$ or 0·1875	14,590	13,230
⅝ or 0·625	$\frac{3}{16}$ or 0·1875	15,420	13,750
⅝ or 0·625	⅛ or 0·125	10,670	9,320
⅝ or 0·625	⅛ or 0·125	11,730	9,580

It will be seen from the above, that the punched bars had the greatest strength, indicating that punching had the effect of strengthening instead of weakening the metal. These experiments have given results just the reverse of similar experiments made on boiler plates; but the material, such as above experimented upon, is what should be placed in boilers, tough and ductile, and the manner of, and care taken in, punching, contribute to these results.

It is usual to have the rivet holes one-sixteenth of an inch in diameter larger than the rivets, in order to allow for their expansion when hot; it is evident, however, that the difference between the diameters of the rivet hole and of the rivet should vary with the size of the rivet.

The hole in the die is made larger than the punch; for ordinary work the proportion of their respective diameters varies from 1 : 1·5 to 1 : 2.

As I have before stated, the best plate joint is that in which the strength of the plate and the resistance of the rivet to shearing are equal to each other.

In boilers as commercially made and sold the difference in quality of the plates and rivets, together with the great uncertainty as to the exact effect of punching the plates, have, so far, prevented anything like the determination either by calculation or experiment of what might be accepted as the best proportions of riveted joints.

In regard to steel plates for boilers Mr. F. W. Webb, of Crewe, England, Chief Engineer of the London and North-western Railway, has made over 10,000 tests of steel plates, but had only two plates fail in actual work; these failures he thought were attributable solely to the want of care on the part of the men who worked the plates up.

All their rivet holes for boilers where punched in a Jacquard machine, the plates then annealed, and afterwards bent in rolls; they only used the reamer slightly when they had three thicknesses of plate to deal with as in butt-joints with inside and outside covering strips. These works turn out two locomotive boilers every three days.

The Baldwin Locomotive Works, which turn out on an average three locomotives per day, punch all their rivet holes one-sixteenth inch less in diameter and ream them to driven rivet size when in place. They also use rivets with a fillet formed under head made in solid dies.

Rivets.—Rivets of steel or iron should be made in solid dies. Rivets made in open dies are liable to have a fin on the shank, which prevents a close fit into the holes of the plates. The use of solid dies in forming the rivet insures a round shank, and an accurate fit in a round hole. In addition,

there is secured by the use of solid dies, a strong, clean fillet under the head, the point where strength is most needed.

Commencing with a countersunk head as the strongest form of head, the greater the fillet permissible under the head of a rivet, or bolt, the greater the strength and the decrease in liability to fracture, as a fillet is the life of the rivet.

If rivets are made of iron, the material should be strong, tough and ductile, of a tensile strength not exceeding 54,000 pounds per square inch, and giving an elongation in *eight inches*, of not less than twenty-five per cent. The rivet iron should be as ductile as the best boiler plate when cold.

FIG. 15.

FIG. 16.

Iron rivets should be annealed and the iron in the bar should be sufficiently ductile to be bent cold to a right angle without fracture. When heated it should be capable of being flattened out to one-third its diameter without crack or flaw.

If rivets are made of steel they must be low in carbon, otherwise they will harden by chilling when the hot rivets are placed in the cold plates. Therefore, the steel must be particularly a low grade or mild steel. The material should show a tensile strength not greater than 54,000 pounds per square inch, and an elongation in *eight inches* of thirty per cent. The United States Government requirements are that steel rivets shall flatten out cold under the hammer to

the thickness of one-half their diameter without showing cracks or flaws; shall flatten out hot to one-third the diameter, and be capable of being bent cold in the form of a hook with parallel sides without cracks or flaws. These requirements were thought at first to be severe, but the makers of steel now find no practical difficulty in meeting these specifications.

The forming of the head of rivets, whether of steel or iron, and whether the heads are conical or semi-spherical, should not be changed by the process of riveting. The form of the head is intended to be permanent, and this permanent form can only be retained by the use of a "hold-fast," which conforms to the shape of the head. In the use of the flat hold-fast (in general use in a majority of boiler shops) the form of the head is changed, and if the rivet, by inadequate heating, requires severe hammering, there is danger that the head of the rivet may be "punched" off. By the use of a hold-fast made to the shape of the rivet head, this danger is avoided and the original form of the head is retained. This feature of the use of proper rivet tools in boiler shops has not received the attention it deserves. Practical use of the above-named hold-fast would soon convince the consumers of rivets of its value and efficiency.

The practice of driving rivets into a punched rivet hole from which the fin or cold drag, caused by the movement of the punch, has not been removed by reaming with a countersunk reamer, or better still a countersunk set, should be condemned, as by driving the hot rivet head down against the fin around the hole in the cold plate caused by the action of punching, the countersunk fillet is not only destroyed, but it is liable to be driven into the head of the rivet, partially cutting the head from the shank. If the rivet is driven into a hole that has been punched with a sharp punch and sharp die, the result is that the fillet is cut off under the head, and the riveted end is also cut, and does not give the clinch or hold desired. That is to say, rivet holes in plates to be riveted should have the burr or sharp edge taken off, either by countersinking, by reamer, or set.

Heating of rivets.—Iron rivets are generally heated in an ordinary blacksmith's or rivet fire having a forced blast; they are inserted with the points down into the fire, so that the heads are kept practically cool.

Steel rivets should be heated in the hearth of a reverberatory furnace so arranged that the flame shall play over the top of the rivets, and should be heated uniformly throughout the entire length of the rivet to a cherry-red. Particular attention must be given to the thickness of the fire in which they are heated.

Steel, of whatever kind, should never be heated in a thin fire, especially in one having a forced blast, such as an ordinary blacksmith's or iron rivet furnace fire. The reason for this is that more air passes through the fire than is needed for combustion, and in consequence there is a considerable quantity of free oxygen in the fire which will oxidize the steel, or in other words, burn it. If free oxygen is excluded steel cannot burn; if the temperature is high enough it can be melted and will run down through the fire, but burning is impossible in a thick fire with a moderate draft.

This is an important matter in using steel rivets and should not be overlooked; the same principle applies to the heating of steel plates for flanging.

Riveting.—There are four descriptions of riveting, namely:

(1) Hammered or hand-riveting;

(2) Snapped or set;

(3) Countersunk;

(4) Machine.

For good, sound work, machine riveting is the best.

Snapped riveting is next in quality to machine riveting.

Countersunk riveting is generally tighter than snapped, because countersinking the hole is really facing it; and the countersunk rivet is, in point of fact, made on a face joint. But countersinking the hole also weakens the plate, inasmuch as it takes away a portion of the metal, and should only be resorted to where necessary, such as around the front of furnaces, steam chests or an odd hole here and there to clear a flange, or something of that sort.

Hammered riveting is much more expensive than

machine or snapped riveting, and has a tendency to crystallize the iron in the rivets, causing brittleness.

In the present state of the arts all the best machine riveters do their work by pressure, and not by impact or blow.

The best machines are those of the hydraulic riveting system, which combines all of the advantages and avoids all the difficulties which have characterized previous machine systems; that is to say, the machine compresses without a blow, and with a uniform pressure at will; each rivet is driven with a single progressive movement, controlled at will. The pressure upon the rivet after it is driven is maintained, or the die is retracted at will.

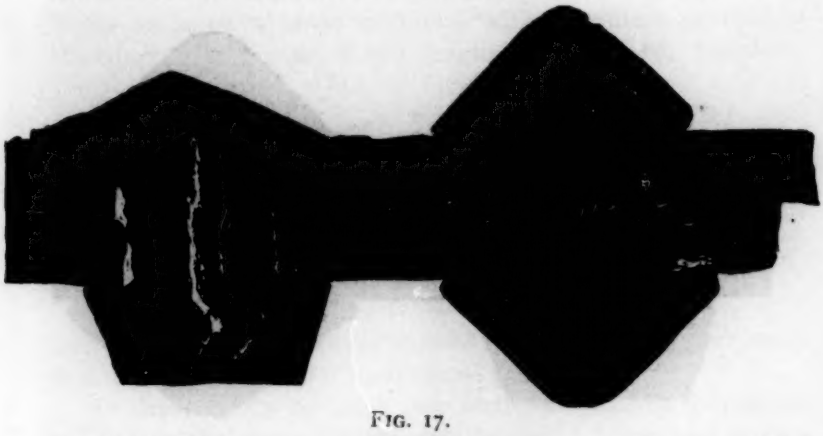

FIG. 17.

Hydraulic riveting has demonstrated not only that the work could be as well done without a blow, but that it could be *better done without a blow*, and that the riveted material was stronger when so secured than when subjected to the more severe treatment under impact.

What is manifestly required in perfect riveting is, that the metal of the rivet while hot and plastic shall be made to flow into all the irregularities of the rivet holes in the boiler sheets; that the surplus metal be formed into heads as large as need be, and that the pressure used to produce these results should not be in excess of what the metal forming the boiler shall be capable of resisting.

It is well known that metals, when subjected either cold or hot, to sufficient pressure, will obey almost exactly the same laws as fluids under similar conditions, and will flow into and fill all the crevices of the chamber or cavity in which they are contained. If, therefore, a hot rivet is inserted into the holes made in a boiler to receive it, and is then subjected to a sufficient pressure, it will fill every irregularity of the holes, and thus fulfil one of the conditions of perfect riveting. This result it is impossible to accomplish with perfection or certainty by ordinary hand riveting, in doing which the intermittent blows of an ordinary hammer are used to force the metal into the holes. With a hydraulic riveting machine, however, an absolutely uniform and con-

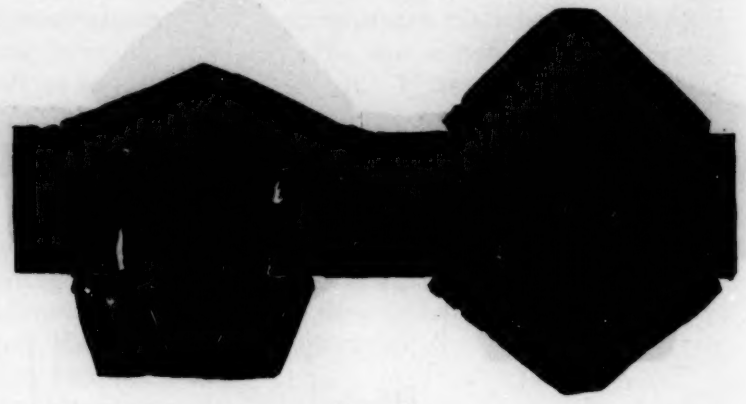

FIG. 18.

tinuous pressure can be imparted to each rivet, so as to force the hot metal of the rivet into all the irregularities of the holes in the same way as a hydraulic ram will cause water to fill any cavity, however irregular.

In order to illustrate the relative advantages of machine over hand riveting, two plates were riveted together, the holes of which were purposely made so as not to match perfectly. These plates were then planed through the centre of the rivets, so as to expose a section of both the plates and rivets. From this an impression was taken with printer's ink on paper and then transferred to a wooden block, from which *Figs. 17* and *18* were made.

The machine-driven rivet is marked *a*, and *b* represents the hammered rivet.

It will be observed that the machine rivet fills the hole completely, while the hand rivet is very imperfect. This experiment was tried several times, with similar results each time.

The hand rivet, it will be observed, filled up the hole very well immediately under the head formed by the hammer; but sufficient pressure could not be given to the metal—or at least it could not be transferred far enough—to affect the metal at some distance from the driven head. So great is this difficulty that in hand riveting much shorter rivets must be used, because it is impossible to work effectively so large a mass of metal with hammers as with a machine. The heads of the machine rivets are, therefore,

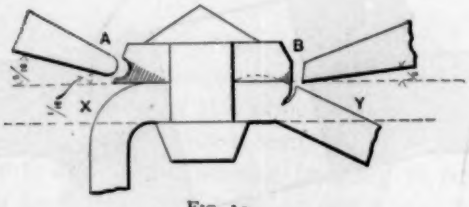

FIG. 19.

larger and stronger, and will hold the plates together more firmly than the smaller hammered heads.

To drive rivets by hand, two strikers and one helper are needed in the gang, besides the boy who heats and passes the rivets; to drive each five-eighths-inch rivet, an average of 250 blows of the hammer is needed, and the work is but imperfectly done. With a machine, two men handle the boiler, and one man works the machine; thus, with the same number of men as is required in riveting by hand, five rivets are driven each minute.

The superior quality of the work done by the machine would alone make its use advantageous; but to this is added greatly increased amount of work done.

The difference in favor of the riveting machine over hand riveting is at least *ten* to *one*.

In a large establishment a record of the number of rivets

driven by the hand-driving gang, also by the gang at the steam-riveting machine for a long period of time, in both cases making no allowances of any kind for delays. The rivets driven per month by each was—for the hand-driven rivets, at the rate of twelve rivets per hour, and for the machine-driven rivets, 120 per hour. In the case of the hand-driven rivets the boiler remains stationary and the men move about it, while the machine-driven rivets require the whole boiler to be hoisted and moved about at the riveting machine to bring each hole to the position required for the dies. Notwithstanding the trouble involved in handling and moving the boiler, it shows that it is possible

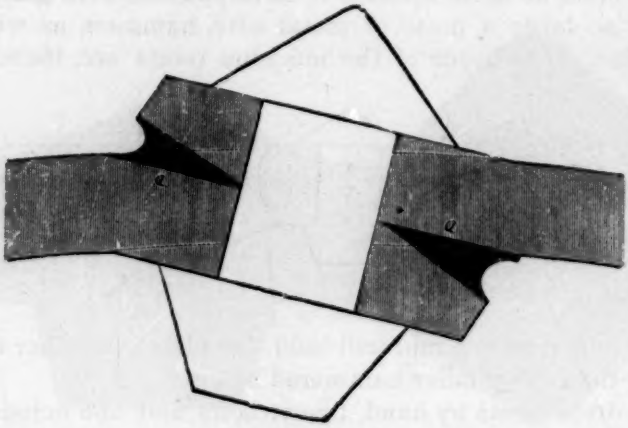

FIG. 20.

to do ten times as much work, and with less skilled labor, by the employment of the riveting machine.

Calking.—One great source of danger in boiler making is excessive joint calking—both inside and out—where a sharp-nosed tool is employed, and for the reason that it must be used so close to the inner edge of plate as to indent, and in many cases actually cut through the skin of the lower plate. This style of calking puts a positive strain upon the rivets, commencing distortion and putting excessive stress upon rivets—already in high tension before the boiler is put in actual use. It is, I hope, rapidly becoming a thing of the past.

With a proper proportion of diameter and pitch of rivet all that is required is the use of a light "fuller tool" or the round-nosed tool used in what is known to the trade as the "Connery system."

There is but little need of calking if means are taken to secure a clean metal-to-metal face at the joint surfaces. When the plates are put together in ordinary course of manufacture, a portion of the mill scale is left on, and this is reduced to powder or shaken loose in the course of riveting and left between the plates, thus offering a tempting opening for the steam to work through, and is really the cause of the heavy calking that puts so unnecessary a pressure on both plate and rivet. A clean metallic joint can be secured by passing over the two surfaces a sponge wet with a weak solution of sal-ammoniac and hot water, an operation certainly cheap enough both as to materials and labor required.

The above cut, *Fig. 19*, gives an illustration of calking done by sharp-nosed and round-nosed tools, respectively. It will be seen by *Fig. 20* that the effect of a round-nosed tool is to divide the plate calked, and as the part divided is well driven towards the rivets, a bearing is formed at *a*, from one-half to three-fourths of an inch, which increases the strength of joint, and will in no way cut or injure the surface of the under plate. A perfect joint is thus secured.

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THE LAW OF VARIATION

OF THE THEORETICAL AMPLITUDE OF TIDAL OSCILLATION, AND THE ELEVATION OF THE HIGH WATER LINE IN THE VARIOUS CROSS-SECTIONS OF TIDAL RIVERS.

BY L. D'AURIA.

The problem of the variation of tidal oscillation or tidal range, from one cross-section to another of a tidal river has never been solved, and what is at present known on the subject is summed up in the vague notion that the tidal range generally increases when the width of cross-section diminishes.

In the *United States Coast Survey Report of 1854*, on pages 171, 172, can be found the statement that "the height of the tidal wave in a channel of the form of a wedge, and of equal depth increases nearly in the inverse ratio of the square root of width;" a law which, were it true, would produce an infinitely great tidal range at the head of any tidal river which would happen to terminate into a point, irrespectively of either the length or the depth of such a river. Of course, we know that this is not in the least the law of nature, and we do not need therefore take further notice of it. The reference has been make only to show how confused and meagre the literature of the subject is, without any intention to criticise.

In this investigation the following two hypotheses or propositions are involved:

(1) The tidal range in any given cross-section of a tidal river is supposed to remain unchanged were such cross-section completely occupied by a dam.

(2) The volume of water confined between any two given cross-sections of a tidal river at the instant when high water occurs, either in one or in the other of these cross-sections is the same.

These two propositions are, no doubt, admissible when the river is supposed to be of a uniforn width, and there seems to be no reason why they should not hold good also for rivers of variable width.

Basing upon this assumption let MM and NN (*Fig. 1*), represent two cross-sections of a tidal river, and let $MM_1 = R$ represent the tidal range of the lower cross-section MM; $NN_1 = \rho$ the theoretical tidal range of the upper cross-section NN; $M_1M_2 = y_1$ the elevation of tide in the lower cross-section at the instant when high water occurs in the upper cross-section; L the distance between the two cross-sections; A the area of the basin enclosed between them; and x_0 the distance of the centre of gravity of such basin

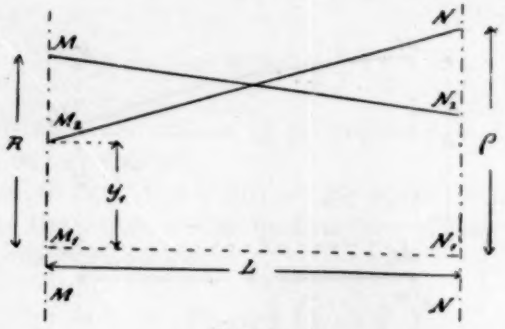

FIG. 1.

measured from the lower cross-section MM. Then the volume of water $NM_2M_1N_1$ can be expressed by

$$q = Ay_1 + A\frac{x_0}{L}\left(\rho - y_1\right) \qquad (1)$$

In accordance with the second proposition above enunciated this volume should be equal to the volume $MN_2N_1M_1$, and this, according to the first proposition, ought to be equal to the tidal volume that may be admitted up to the time of high water into a tidal river of length L, and area A, through the cross-section MM. This latter tidal volume can be computed by considering the tidal basin of uniform width b equal to the width of the lower cross-section, and of length $z = A \div b$. (See "Analytical Discussion of the Tidal

Volume," etc., by the author, *Journal Franklin Institute*, April, 1891). Let then y represent the ordinate of the tidal wave (*Fig. 2*), in this ideal tidal river at the distance z from the crest M, the height of the wave being R. Then, with the same degree of approximation as adopted in the previous computation the required tidal volume can be expressed by

$$q = \tfrac{1}{2} A (R + y) \qquad (2)$$

The value of y is given by the equation

$$y = R \sin^2 \left(\frac{\pi x}{\lambda}\right)$$

in which x represents the distance from the trough of the

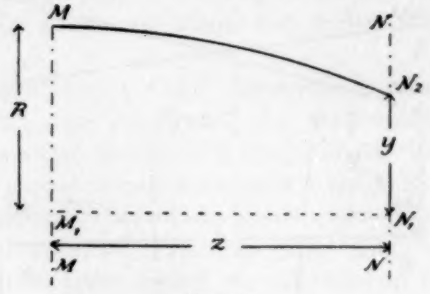

FIG. 2.

wave, or $x = \frac{1}{2}\lambda - z$. By substitution and reduction we obtain

$$y = R \cos^2 \left(\frac{2z}{\lambda}\right)\frac{\pi}{2}$$

Since $z = A \div b$, if we denote by b_0 the average width of the basin we get

$$z = L \frac{b_0}{b}$$

and

$$y = R \cos^2 \left(\frac{2 L b_0}{\lambda b}\right)\frac{\pi}{2}$$

Now, if we denote by θ the time occupied by the wave crest to travel the distance L, and by T the mean interval

of rise and fall of tide, which is about 372 minutes, we can put

$$\frac{2\,L}{\lambda} = \frac{\theta}{T}$$

and

$$y = R\,cos^2\left(\frac{\theta}{T}\frac{b_0}{b}\right)\frac{\pi}{2} \qquad (3)$$

Substituting this value in equation (2) and then comparing with equation (1), we find

$$\rho = \frac{R\,L}{2\,x_0}\left\{1 + cos^2\left(\frac{\theta}{T}\frac{b_0}{b}\right)\frac{\pi}{2}\right\} - y_1\left(\frac{L}{x_0} - 1\right) \qquad (4)$$

When the basin is rectangular, we have $b_0 = b$, $2\,x_0 = L$, and

$$y_1 = y = R\,cos^2\left(\frac{\theta}{T}\right)\frac{\pi}{2}$$

Substituting these values in (4) we get $\rho = R$, a result which was to be expected.

If we denote by b_1 the width of the upper cross-section, and express the width of the basin at any distance x from the lower cross-section by

$$\gamma = b_1 + \left(b - b_1\right)\left(1 - \frac{x}{L}\right)^n$$

then

$$x_0 = \frac{\int_0^L \gamma\,x\,dx}{\int_0^L \gamma\,dx}$$

and

$$b_0 = \frac{1}{L}\int_0^L \gamma\,dx$$

Substituting the value of γ, and integrating, we find

$$x_0 = \frac{L}{2}\,\frac{2\,b + n\,b_1\,(n+3)}{(n+2)\,(b + n\,b_1)} \qquad (5)$$

and

$$b_0 = \frac{b + n\,b_1}{n+1} \tag{6}$$

With these values equation (4) becomes

$$\rho = \frac{R\,(n+2)\,(b+n\,b_1)}{2\,b + n\,b_1\,(n+3)} \left[1 + cos^2 \left\{ \frac{\theta}{T} \frac{b + n\,b_1}{(n+1)\,b} \right\} \frac{\pi}{2}\right] - y_1 \frac{(1+n)\,(n\,b_1 + 2b)}{2\,b + n\,b_1\,(n+3)} \tag{7}$$

When the basin is trapezoidal in shape, then $n = 1$, and we have

$$\rho = \frac{3\,R\,(b+b_1)}{2\,(b+2\,b_1)} \left[1 + cos^2 \left\{ \frac{\theta}{2\,T}\left(1 + \frac{b_1}{b}\right) \right\} \frac{\pi}{2}\right] - y_1 \frac{2\,b + b_1}{2\,b_1 + b} \tag{8}$$

and when the basin is triangular in shape, $b_1 = o$, and

$$\rho = \tfrac{2}{3}\,R \left\{ 1 + cos^2 \left(\frac{\theta}{2T} \right) \frac{\pi}{2} \right\} - 2\,y_1 \tag{9}$$

In all these formulas θ is supposed not to exceed the value of T; but suppose we have the case of $\theta = T + \theta_1$ then we have to find first the value of ρ which corresponds to $\theta - \theta_1 = T$, and afterwards, with this value of ρ put instead of R and θ_1 put instead of θ, our equations will furnish the value of ρ corresponding to θ. In practice, however, in order to ascertain the variation of the theoretical tidal oscillation from cross-section to cross-section in a tidal river we have to divide such river in sections of such length that the curvature of the tidal wave in each section may be neglected.

When $\theta = T$, the value of y_1 becomes zero, and the formulas (7), (8) and (9) become, respectively,

$$\rho = \frac{R\,(n+2)\,(b+n\,b_1)}{2\,b + n\,b_1\,(n+3)} \left(1 + cos^2 \left\{ \frac{b + n\,b_1}{(n+1)\,b} \right\} \frac{\pi}{2}\right) \tag{10}$$

$$\rho = \frac{3\,R\,(b+b_1)}{2\,(b+2\,b_1)} \left(1 + cos^2 \left\{ \tfrac{1}{2}\left(1 + \frac{b_1}{b}\right) \right\} \frac{\pi}{2}\right) \tag{11}$$

and

$$\rho = \frac{9}{4} R \tag{12}$$

If we put $b_1 = o$ in equation (10) we get

$$\rho = \tfrac{1}{2} R (n+2) \left\{ 1 + \cos^2 \left(\frac{1}{n+1} \right) \frac{\pi}{2} \right\} \tag{13}$$

which clearly shows that ρ increases with n. Hence, in a trumpet-shaped tidal river or estuary, the range of tide increases more rapidly than if such river or estuary were triangular.

When the value of b in equation (4) cannot be accurately determined, owing to sudden and irregular variations of width, then it will be better to substitute in such equation the value of $\frac{b_0}{b}$ expressed in function of x_0 and L as given by the formulæ (5) and (6), viz:

$$\frac{b_0}{b} = \frac{L}{(n+3) L - 2 (n+2) x_0} \tag{14}$$

and we get

$$\rho = \frac{R L}{2 x_0} \left\{ 1 + \cos^2 \left(\frac{\theta}{T} \frac{L}{(n+3) L - 2 (n+2) x_0} \right) \frac{\pi}{2} \right\} - y_1 \left(\frac{L}{x_0} - 1 \right) \tag{15}$$

If the portion of river under consideration be rather short we can put $n = 1$, and then we have

$$\rho = \frac{R L}{2 x_0} \left\{ 1 + \cos^2 \left(\frac{\theta}{2T} \frac{L}{2L - 3 x_0} \right) \frac{\pi}{2} \right\} - y_1 \left(\frac{L}{x_0} - 1 \right) \tag{16}$$

When, however, the variation of width in the length L is such as to show a decided curvature, then we can, by few tentatives, find out which value of n better corresponds to it; but in practice it will be found that where the curve presents its convexity to the stream, for a short portion of river, we can put $n = 2$; and when the curve presents its concavity to the stream, we can put $n = \frac{1}{2}$ in equation (15) to obtain the corresponding value of o.

The tidal oscillation ρ, as given by the above formulæ, would obtain in rivers perfectly free of resistances, and would take place one-half above and one-half below a common level plane for the whole length of the river. As it is, however, there is always a greater or less volume of water held back during the ebb discharge, on account of the various resistances of the river bed, and this volume of water, without affecting the position of the high-water line, has the effect of raising the low-water line above its theoretical position. Now, it is easy to see that the difference of elevation of high-water between any two given cross-sections of a tidal river can be computed by the formula

$$\delta = \tfrac{1}{2}(\rho - R) \qquad (17)$$

which offers a simple and accurate method of running a level line along any tidal river entirely by means of tidal observations.

The writer had in mind to test the accuracy of this method by comparison with some well-established level line along a tidal river, but a sudden change in his professional occupation having made it difficult for him to get at the necessary data this part of his work had to be postponed for some future time. The following example, however, though only computed with roughly estimated data, may serve to give some idea of the plausibility of the results which may be expected from the above method.

In 1870, the United States Coast Survey, by means of levels of precision, determined the elevation of mean level (half tide) at League Island Navy Yard, on the Delaware River, above the mean level (half tide) at Keyport, N. J., and it was found to be 3·3 feet. The mean level at the entrance of Delaware Bay can be considered practically the same as that at Keyport; and since the range of tide at League Island is given at 6·25 feet, and at the Delaware Bay as nearly 5·0 feet, we would have for the difference of elevation of high water between League Island and Dela ware Bay,

$$\delta = 3{\cdot}3 + \frac{6{\cdot}25}{2} - \frac{5}{2} = 3{\cdot}92 \text{ feet.}$$

From the entrance of Delaware Bay up to League Island the value of θ is nearly equal to T, so that we can put $y_1 = o$ in equation (4) which then gives

$$\rho = \frac{R\,L}{2\,x_0}\left\{1 + \cos^2\left(\frac{b_0}{b}\right)\frac{\pi}{2}\right\}$$

The values of

$$\frac{L}{x_0} \text{ and } \frac{b_0}{b}$$

have been approximately estimated at four and two-thirds, respectively, which offer

$$\rho = 2{\cdot}5\ R$$

Substituting in equation (17) we get

$$\delta = 0{\cdot}75\ R$$

and since $R = 5$ feet, we have

$$\delta = 3{\cdot}75 \text{ feet.}$$

This differs from the value deduced from the Coast Survey levels of precision for less than two-tenths of a foot.

Denoting by ρ_1 the actual observed value of ρ we can form some idea of the efficiency of the river bed so far as the freedom of flow is concerned, by consulting the following ratio or coëfficient of efficiency, viz:

$$\varepsilon = \frac{\rho - \rho_1}{\rho} \qquad (18)$$

In the case of the Delaware River just considered, the theoretical range of tide would be $\rho = 12{\cdot}5$ feet. Hence, with $\rho_1 = 6{\cdot}25$ feet we get $\varepsilon = 0{\cdot}5$.

The removal of obstructions and the deepening of the channel of a tidal river have the tendency of bringing ε nearer to *one*: but in no case such limit can be practically reached. When such improvements are effected on a tidal river, the value of ρ_1 is increased by the lowering of the actual low-water line toward its theoretical position without affecting the position of the high-water line. This conclusion is found to be corroborated by a wide experience in the improvement of tidal rivers.

THE LIMITS OF SCIENTIFIC INQUIRY.

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I. GENERALITIES AND DEFINITIONS.

It is a peculiarity of natural science that its most surprising revelations are to be found in quarters where they are often least suspected, and even the most dissimilar departments of inquiry are so interlinked, so closely connected by invisible threads, as it were, that it is almost a matter of indifference what object we take in hand, whether a butterfly, or the first pebble we come across; a piece of chalk, or a piece of charcoal; a meteorite, or a star-fish, we shall find a sermon in it. We shall find—if we carry our inquiry far enough—that the butterfly and the pebble are not so very different in kind after all; that there is more than a superficial connection between a piece of chalk and one of charcoal, and that even such dissimilar things as meteorites and star-fishes show points of analogy where we may have least expected them.

We are living in a world of mystery. We are surrounded by appearances which we fondly persuade ourselves to be realities, having from infancy been accustomed to look upon them as such, though they are little more than phantoms of our imagination, or, as Carlyle aptly expressed it, the clothes in which we dress our ideas. On the other hand, there are a number of phenomena which, by reason of their very grandeur, should inspire us with awe, and give us food for unceasing reflection. We see the sun rise in the east and set in the west, and look upon this as the most natural thing in the world; we see the moon with its changes, and all the starry host—shooting-stars, and now and then a comet, and habit has rendered us indifferent to these phenomena. We drop a stone and expect it to fall upon the ground, we plant an acorn and expect an oak-tree from it: we would be greatly surprised, indeed, to see it develop into

a cucumber; we perceive the effects of heat and cold upon ourselves and other bodies, and look upon all these things as matters of course—yet they are profound and awful mysteries if we look below the surface.

Carlyle, in his *Sartor Resartus*, gives utterance to the following remarkable sentence: "I stretch forth my hand to clutch the sun. Vain and foolish attempt! It is 100,000,000 miles distant. Yet, if by some wondrous miracle, my arm were to extend, of a sudden, so that I could touch it, the marvel would merely consist in the sudden lengthening of my arm, and in the power of my body to sustain it: it would be incomparably smaller than the marvel that I am here and have an arm which I can stretch forth at all."

We are surrounded by mysteries. Our existence itself is a mystery, and in proportion as we recognize this fact, we become restless and dissatisfied. We experience an indescribable longing to solve these mighty problems, to grasp the why and how of things, to fathom the infinite—a yearning and striving which not seldom becomes a burning desire, and develops into an all-absorbing passion. This feeling exists, it is real, and is implanted, I venture to say, in every human heart, although the majority may be unconscious of it. It manifests itself not only in the speculations of the philosopher, but in the general thirst for knowledge—in the curiosity of women as well as in the inquisitiveness of children. This universal longing for the unknown, this insatiable desire to ascertain facts and draw conclusions: what is it but our innate tendency to solve the riddle of existence, which seems to be the common inheritance of mankind?

The riddle of existence has not been, and probably never will be, completely solved, although it would be an unwarrantable dogmatical assertion to pronounce as absolutely impossible so gigantic a triumph of the human mind. But it may comfort us to know that we are at last marching in the right direction. We are no longer groping blindly in the dark, and, though we may never reach the goal, yet we can approach it. We can approach that glorious goal,

and we are approaching it. Our progress during the last 100 years is not merely visible, it is vast. It is marked by a succession of the most wonderful discoveries, of marvellous truths, which have flashed upon us; which have widened our horizon beyond the wildest dreams of eighteenth-century philosophers, and there is every indication that this progress will continue in a geometrical ratio.

2. THE OLD AND NEW PHILOSOPHY.

Our methods of inquiry differ vastly, if not totally from those of old, from those pursued by the old-world philosophers, with few exceptions, from Plato to Descartes. We take nothing for granted. We do not raise our fabric on a dogmatical assumption however seductively it may allure us. We draw our conclusions from experience, from facts which may be tested and verified at any moment: we require no belief, and we scorn to appeal to unreasoning "faith" of any description.

The difference between the old and new method is this: The inquirers and philosophers of the past imagined that they could arrive at the truth by a mere process of reasoning, independent of the phenomena of nature which surrounded them. They based their ingenious structures on the monstrous assumption that thought or mind existed altogether distinct from matter, that mind and matter were two separate existences, which have nothing whatever in common. They believed that by mere mental efforts they could solve the secrets of the universe, independent of matter, its laws and properties. Hence a science in our sense was, with them, impossible. They treated the phenomena of nature with indifference, nay with undisguised contempt, as unworthy of their notice, they relied solely on the "mind" to account for all that is hidden. Thus all the systems which they compiled, based on this rotten foundation, however ingenious in themselves, are monstrous absurdities, quite astounding and amusing if examined in the light of modern science. They are interesting only in so far as they afford an appalling example of misdirected and lost energy, and by warning us to avoid the rocks on which they grounded.

We have abandoned this hopeless dominion; we have struck out a road for ourselves. We do not believe that mind and matter are separate existences—in fact, we do not "believe" anything. To us the word "belief" is devoid of meaning; we either know or are in a state of agnosis. We have strong grounds for assuming that what is called "mind" is only a property of matter, just as elasticity, expansibility and gravity are properties of matter, and it is to matter that we are looking for redemption. We do not analyze and dissect the mind, we analyze and dissect matter. The modern philosopher studies physiology before he ventures an opinion on psychology: witness Herbert Spencer.

We observe the laws and phenomena of nature, we investigate the character of the objects which surround us and draw from them our conclusions. We know and recognize no other guide. If our deductions are hostile to theology the hostility is not intentional on our part, inasmuch as it is not, from the outset, our motive to destroy religious beliefs. If our labors should tend to do so, we can only shrug our shoulders. We want to proclaim the truth, quite irrespective of the consequences; we have nothing whatever to do with this subject in the prosecution of our task, we leave it entirely alone. To say, for instance, there is no creator, is in our eyes quite as dogmatical as to say there is one, but we decline to resort to the supposition of a deity in order to account for those mysteries which have, as yet, eluded our grasp. If, in order to account for the origin of a world, the disciple of religion points to a deity we only remind him that by doing so he explains nothing. He only substitutes a greater mystery for one already appalling in its magnitude. He reminds us of the Hindoo fable in reference to the "foundations of the universe," expressed in stone on every Hindoo temple. We there behold the figure of an elephant, carrying on its back a huge disc, which represents the world. The elephant, again, is supported by a still more gigantic tortoise. The speculative Hindoo is not so much interested in the question of the world's creation as in the problem of its foundation or support.

"What does the world rest on?" we ask a Brahmin.

"An elephant."

"And the elephant?"

"On a tortoise."

"And the tortoise?"

"That supports itself; it rests on nothing."

Supports itself? rests on nothing? Would it not have been much simpler to make the world self-supporting at once? Why introduce the elephant and the tortoise? They explain nothing; on the contrary, they only increase the world's weight and our difficulty.

Modern science discards every species of dogma and ignores every kind of supernatural intervention. Modern science is not conservative and pedantic, it is progressive and courts publicity; it is open to all and offers the purest and noblest pleasures, while it elevates the mind and makes veritable kings of its disciples.

3. THE GROWTH OF SCIENTIFIC KNOWLEDGE.

Geology is one of the most charming of the natural sciences, but, at the same time, it is one of the most difficult, because it involves questions and problems of the most varied and complicated character. Extensive preliminary studies in the departments of mineralogy, chemistry, physics and even astronomy are necessary before a broad general conception of this great science can be acquired by the student. There was a time—less than fifty years ago—when even great and wealthy colleges employed only one man to teach the whole of the natural sciences. To be sure they always had their full complement of historians, theologians, professors of literature, and, above all things, a complete set of philologists to take care of those very tremendous subjects, Greek and Latin. They were very expensive, these "classical" languages, but then they possessed the advantage of being comparatively useless which in itself was sufficient to insure them the highest esteem of the average university-regent.

The natural sciences were relegated to one man, generally poorly paid, who was styled "Professor of Natural Science,"

and who was expected to teach, and know all about, physics, chemistry, astronomy, geology, biology; in short, everything which comes within the scope of natural science. At the present day each of these subjects, and a dozen others, forms a distinct and separate study, and the labors of an army of indefatigable workers in each have already resulted in the accumulation of an amount of scientific literature, the mastering of which even in its most essential features, taxes the endurance of the most persevering.

To-day we have no professors of natural science at our large colleges, but we have professors of astronomy, of geology, of mineralogy, of chemistry, physics, etc., for each of these subdivisions has grown into a great science for itself, which requires the study of a life-time. The science of geology alone has assumed such vast proportions that no ordinary intellect can master it in all its bearings. Thus we now have palæontologists, men who devote themselves entirely to the study of fossils, mineralogists who inquire into the forms and properties of minerals, and petrographers who examine into the structure of rock-masses, and each of these specialties is capable of still further subdivision, and where it will end is more than I dare conjecture. If we go to any of our great colleges we shall find that the professor of botany, for instance, knows comparatively little beyond his own specialty; his knowledge of the other departments of natural science is sometimes not much better than that of the average school-boy's. It is sad that it should be so, but it is the inevitable consequence of the growth of knowledge. We can no more expect a professor of botany to know all about geology, chemistry, or natural philosophy, than we can expect a pint measure to hold a bushel. These men are the intellectual hewers of wood and drawers of water, the compilers of raw material, and it happens only once or twice in a century that a master-mind appears, like Laplace or Isaac Newton, like Darwin or Herbert Spencer, who surveys the whole gigantic field, like some general, who connects the accumulated facts and links them, and traces a wonderful law, of which no one had dreamed, which is listened to by mankind with bated breath, and received as a new and glorious revelation.

4. GEOLOGICAL PARADOXES.

We will now endeavor to present or discuss one or two problems in reference to the structure of our earth, or better, perhaps, in reference to the condition of the earth's interior, which may be new to many, and which, if they are not altogether new to those who have studied geology in its broader aspects, in connection with astronomy, physics and chemistry, will at least interest them in so far as the writer may succeed in giving an explanation of certain phenomena which have not as yet, to his knowledge, been accounted for in popular language. Besides, it may be presumed that an object looks different according to the direction in which we view it, and the greater the number of these directions, and the more varied the points of view the more perfect and accurate will be our impression of the size, shape and character of that object.

This globe, our earth, which we inhabit, and which revolves once in twenty-four hours around its axis, while pursuing at the same time its great course around the sun, and onward with the entire solar system towards an unknown region of space—probably around some still greater centre of attraction—this earth of ours has a diameter of about 8,000 miles. We live on its surface, on the surface of its outer crust, as it has been termed. We are tolerably well acquainted with this surface; we have travelled nearly all over it and have explored it in every direction, even though the highest polar regions and some parts of Central Africa are still unknown to us. We know that about three parts consist of water, in the shape of oceans and seas, and one part of what we are accustomed to call land. We find on this land mountains and valleys, and strange configurations, we find it rising from the ocean level, higher and higher, until it reaches in the loftiest summits with which we are acquainted an elevation of nearly 30,000 feet. We find this land, on the other hand, depressed in certain localities, depressed hundreds of feet below the sea-level (the dry land, of course; the coast of the Black Sea is about 200 feet beneath the level of the Mediterranean and the shore of the Dead Sea, 1,000 feet lower still). We have bored and dug

into it to a depth of some thousands of feet, yet our acquaintance with this terrestrial globe is but an exceedingly slight one; it only extends to a most insignificant portion of its outer crust. What are the highest mountains and what are the deepest borings, compared with the enormous bulkof our planet; what is 30,000 feet; what is 100,000 feet, compared with the radius of 4,000 miles? The loftiest Himalaya peaks, if contrasted with the diameter of the earth are but as the little protuberances on the skin of an orange, or the tiny granules on an egg-shell, and if we were to represent the Himalaya range on a globe a foot in diameter a few grains of rice, placed side by side, would furnish more than all the height required.

What is the character of that unknown interior? What kind of materials are hidden in these immense regions below and concealed from our sight forever? This is one of the fundamental problems of geology, a problem which has occupied the minds, not only of geologists but of physicians and astronomers for more than a century. We cannot get at those substances of our earth's interior which are more than a mile or two removed from the surface, and shall probably never be able to ascertain their condition by direct observation; yet we know that these substances must be quite different from those which we find at the surface, viz: in the insignificant portion of the outer crust, which we have been able to explore. It has been found that the average density of the earth is a little over five and a half, in other words the earth weighs about five and a half times as much as a globe of the same size, but consisting of water, would weigh. Now, it is certainly curious that the average gravity of the crust—so far as we have been able to explore it—is only two and a half; all the different kinds of sedimentary, metamorphic and eruptive rocks, minerals, fossils, etc., in short, all the materials of which the outer crust is composed weigh, on an average, only two and a half times as much as water. We are thus driven to the conclusion that the interior of the earth is composed of substances which have more than twice the density of those which constitute the outer crust. So far, we are not dealing

with anything in the nature of a theory or mere speculation, but with facts which can be as clearly proved as that twice two are four.

Now, in order to account for this enormous weight or high specific gravity of the earth, it has been assumed—and there is not now, perhaps, a single progressive geologist who doubts it—that during the gradual cooling or condensation of our globe the heavier substances gravitated towards the centre, and that the others were deposited around these according to their various densities. We are now acquainted with about seventy different fundamental substances or "elements," among which iridium is possessed of the highest specific gravity (weighs twenty-two times more than its bulk of water, thus a little more even than platinum). Now, provided there are not elements still heavier in the vastnesses below, which is extremely probable, we may consider the structure of our planet as that of a series of concentric shells, with a nucleus of iridium. This nucleus may have a diameter of from fifty to 200 miles, it is surrounded by a layer of platinum, followed by gold, lead, silver, copper, iron, etc.—the various metals according to their specific gravity—and finally the lighter elements, such as aluminium, silicon, magnesium and calcium (the latter one of the lightest, its gravity being only one and a half). We may, indeed, safely conclude that this succession of concentric layers, or shell-structure, is the structure of this terrestrial globe, viz: that the various materials are arranged or deposited according to their densities, and it would seem as if the greater part of the earth's interior—the thickest of all the shells—consisted of metallic iron. (It will be readily understood why most, if not all the metallic elements, which constitute the inner shells, must exist in a pure metallic state, if we bear in mind that oxygen, as one of the lightest elements, can only exist on or near the surface, and can only there undergo its combinations.)

It was formerly believed that the earth's interior was in a molten condition, that our planet was, so to speak, a drop of fiery liquid, surrounded by a comparatively thin crust, and the numerous volcanoes, which we find in or on this

crust, were looked upon as the chimneys of the huge inferno, or as safety-valves for letting out steam and other superfluous products. This absurd theory—absurd in the light of modern science—has been abandoned by every philosophical inquirer of the present, and is only retained by a few geologists of the old school, who have written bulky volumes thirty or forty years ago, full of dogmatical assertions, from which—for reasons best known to themselves—they cannot now well depart: men who, during the last thirty years, have neither learned nor forgotten anything, who have not progressed a single inch and still expect to live on the fame which they acquired at the time of our grandfathers, when it was far easier to establish a reputation in science than now.

We can now show, by an overwhelming array of incontrovertible arguments, that the earth must be solid to the very core. We are driven to infer this from the enormous gravity of our planet, from its small diameter, from the necessary increase of pressure towards the centre and from numerous other data. Besides, it has been experimentally demonstrated that when a globular molten mass is allowed to cool the solidification invariably commences at the centre and advances toward the periphery, a fact which had been theoretically established by mathematicians and physicist long before the experiments were undertaken. The lavas and other eruptive products ejected by volcanoes have nothing whatever to do with the earth's interior—with that greater interior which here concerns us—they are derived from an insignificant depth in the crust and are only local phenomena, which will be explained later on.

But here we are confronted by a most remarkable paradox. If we dig down, or bore into, the earth's crust no matter where—we observe an increase of temperature which averages 1° (Centigrade) for every ninety feet. Thus if we go down 180 feet we have 2° more than at the surface, if we go three times ninety, 3° more, etc. The regularity and constancy of this increase of temperature in proportion to the depth is quite astonishing and down to the greatest depth which has, as yet, been reached by boring or mining this law has been

found to prevail. In all the deeper mines, as, for instance, many coal mines in England, Wales and on the continent of Europe, as well as some of the silver mines of Nevada and California the heat is so unbearable that the workers have to be relieved after short intervals.

Now, if this increase of temperature continues at the same rate towards the earth's centre, which cannot well be doubted, then at a depth of less than five miles the heat must already be beyond that of the boiling-point of water, at thirty miles depth every known rock must exist in a state of fusion, at fifty miles even platinum, a metal which we can only melt with the greatest difficulty; at a depth of 200 miles we would have such a fabulous temperature that every known substance could only exist in a most attenuated state, as a gas, and what the condition of matter may be at *2,000* miles depth is more than we dare even hint at; it may be presumed, however, that the explorer would run no risk of catching cold there and that he would get along well without an overcoat.

But how can we reconcile this with the limited diameter and the enormous gravity of our planet? With this gradual increase of temperature the materials, instead of becoming denser and heavier, should become lighter, more and more attenuated or gaseous, in proportion as we approach the central region. This would necessitate a far greater diameter of our globe, a diameter which should extend beyond the moon's orbit, a diameter of at least 500,000, instead of 8,000 miles.

This very remarkable apparent incongruity has been a great puzzle to geologists and physicists, and it was only a few years ago that the mystery was solved in so thorough and simple a manner that many have not yet quite recovered from their astonishment, or rather from mortification in contemplating their previous blindness.

We know that the melting-point, boiling-point or temperature under which any given substance will assume the gaseous state, is determined by the pressure exerted upon it, either by the atmosphere or by any other body. Water, for instance, boils at 100° (Centigrade), but if we ascend

a mountain and heat it there, it will boil earlier, viz: it will require less than 100° of heat to make it boil, and the higher we go the more quickly ebullition will ensue. What is the cause of this?

The atmospheric pressure is less. All the air beneath us—perhaps a stratum a mile or more in thickness—no longer presses on the water, which on that account, can expand and evaporate more readily. On the summit of Mt. Blanc, for instance, water will boil already at 85° (instead of 100°). If we wanted to boil an egg there, we might keep on boiling it for hours, days, years or centuries and it would never be done, because at 85°, the albumen does not coagulate. In proportion as the atmospheric pressure diminishes the boiling-point of water is lowered, and here we have a ready means for determining the heights of mountains. All that is required is a test-tube, a spirit-lamp and a thermometer. A little water is boiled, then, by dipping the thermometer and noting how high the quicksilver column rises, the observer knows at once the approximate altitude. On the Plateau of Peru—perhaps the highest habitable region of our globe—no kind of food can be prepared by means of boiling. We could not cook an egg there, to say nothing of such things as potatoes, rice, peas, etc., neither soups nor stews are possible, the meat would remain almost raw in the water, and if we were to boil it for weeks in succession. Now if, on the other hand, we were to heat water below the level of the ocean, say in a deep mine, it would, of course, require a greater temperature than 100° to cause it to boil, and if we were to enclose it in a cylinder and exert upon it an artificial pressure of several atmospheres, we might expose it to a temperature which would suffice to melt iron, without reaching the boiling-point.

It will now readily be understood why the huge mass of our planet, in spite of the increasing temperature as we approach the centre, must be firm and solid throughout. The heat, of course, increases 1° per ninety feet of depth, but the pressure of each ninety-feet stratum increases in still greater proportion and more than counteracts the

tendency to melt, boil or evaporate, on the part of the materials involved.

Now, if it should happen in the neighborhood of the surface, for lower down it is quite inconceivable that the pressure exerted upon the rock-masses (and which alone prevents them from melting, boiling or flying into space) is temporarily relieved, lightened or removed, in consequence of local disturbances, such as shiftings of strata, denudations, etc., then a certain region which, perhaps, occupied several cubic miles, may be almost instantaneously transformed into a volcanic reservoir. The previously solid mass will be changed into liquid lava which, of course, requires more room and forces its way upwards, causing earthquakes, volcanic outbursts and similar phenomena. The extinct volcanoes along the Rhine, in the Auvergne and at innumerable other regions of the earth's crust, indicate the former existence of such subterranean reservoirs, caused by a release of pressure, in consequence of local disturbances and volcanic regions of this kind may originate at any time and at any place. Such a reservoir requires centuries, or thousands of years, to cool again, as lava is a bad conductor of heat, but ultimately solidification must ensue.

If the vast bulk of our earth's interior were in a molten condition we might naturally expect that the lavas ejected by the numerous volcanoes would exhibit a general uniformity in structure and composition. Instead of this they present the greatest differences, which can only be accounted for by attributing to them a local origin. Thus the lava ejected by Mt. Chimborazo, in the Cordilleras, is a dense black substance, resembling basalt, while Mt. Cotopaxi, which is only a few miles distant, throws out a light porous pumice-stone. Moreover, the specific gravity of the lavas never exceeds three, thus they can only represent regions of the uppermost crust of our planet.

5. IMMORTALITY IN THE LIGHT OF MODERN DYNAMICS.

The hypothesis in reference to the inevitable re-grouping of atoms, in accordance with the calculus of permutations, which I announced in conclusion of my lecture on

"Geological and Cosmical Problems," before the Franklin Institute, on November 17, 1890, is not entirely new, and I am bound to say that in at least one of its aspects it was advanced more than a hundred years ago by the great German philosopher, Leibnitz, at a time when the sciences of chemistry and physics were not sufficiently advanced to warrant such a speculation. In the light of modern dynamics, however, it deserves our closest attention, for if it can be shown that matter is composed of ultimate particles, call them atoms, centres of forces, or what we like, which are indestructible and in a state of continual vibration, I do not see how we can escape the conclusions which are forced upon us by this hypothesis. Some of the points which I am now about to discuss are new and I am not aware that this entire subject has ever been presented in the manner in which I now propose to deal with it.

According to the nebular hypothesis our earth, like all the rest of the planets, once existed in the shape of a gas-ring, which was thrown off, or became detached from, the sun during its process of condensation. This ring could not retain its form, it necessari ly wentto pieces, and these afterwards collected into a single gas-globe, or spherical mass, which kept on pursuing its course around the great central body. The gaseous globe radiated an enormous amount of heat, it grew denser and denser, while its diameter diminished, it underwent an endless series of metamorphoses, until it finally became the earth as we know it, the planet which has given us birth. So far all this is nothing new.

Now, even if the nebular hyphothesis should prove erroneous the conclusions which I am now about to present will remain in force, for the same ultimate conclusions can be drawn from every other world-hypothesis which has, as yet, been advanced.

Every particle of our earth, every object, every substance which we now have upon or in our earth, must have already existed in that gaseous ring or primitive gas-globe, no matter in what form or condition; it was there. In that gas-globe were the particles which, after countless ages, became

united and roamed the great Mississippi valley in the shape of a mastodon, in that globe of gas were the atoms of carbon, which now constitute the table on which I am writing these lines, in that immense rotating sphere were the substances which are now united in the body of my humble self.

Could we but follow, in a few days or hours, the changes, the transformations, the endless pilgrimages, which the atoms and molecules of the substances had to undergo during those æons, before they became united so as to form, for instance, a human body, what marvels would we behold? The particles of hydrogen, carbon, phosphorus, etc., of which my body is composed, what a history might they not tell? In how many other bodies of the human species, of animals, plants and inorganic compounds may they not already have existed, separated, united, differently grouped or arranged? What may they not already have gone through and experienced? The reader will doubtless remember that famous church-yard scene in "Hamlet," in the fifth act, where *Hamlet* picks up a mouldering skull—the skull of poor *Yorick*—and addresses his friend *Horatio* thus:

Hamlet: To what base uses may we not return; why may not imagination trace the noble dust of Alexander till he finds it stopping a bung-hole?

Horatio: 'Twere to consider too curiously to consider so.

Hamlet: No, faith, not a jot, but to follow him thither with modesty enough and likelihood to lead it; as thus: Alexander died, Alexander was buried; Alexander returneth to dust; the dust is earth; of earth we make loam, and why of that loam whereto he was converted might they not stop a beer-barrel?

> "Imperious Cæsar, dead, and turned to clay,
> Might stop a hole to keep the wind away:
> O, that that earth, which kept the world in awe,
> Should patch a wall to expel the winter's flaw!"

If King Solomon, wise king though he was, really pronounced, or was the first to pronounce, the opinion that there is nothing new under the sun, he could not possibly have been aware of the enormous significance which attaches to this idea in the light of modern science. Why should not

the dust of Cæsar, which is now filling a bung-hole, why should not those atoms and molecules which, 2,000 years ago, were united in the body of Cæsar, why should they not, after endless transformations, endless changes, end less transitions, become again united in precisely the same manner; in other words, why should not the same Cæsar of whom we read in ancient history, reappear at a given time: in short, why should not everything now existing be compelled to undergo the same cycle of changes and reappear, not once, but an infinite number of times? It would be very strange if such were not the case. The following will illustrate this:

Supposing we were to take six dice, such as are used in the ordinary game. Let us place them in a little box; shake them and throw them on the table. We will assume that they had fallen so that each cube exhibited the number three on its upper face; of course, a rare chance. Now it can be mathematically shown after how many throws those six numbers are likely to reappear according to the law of chance. It is possible that they may turn up already with the next throw; on the other hand, we may have to cast those dice 10,000 times. Both cases are improbable; the probability lies in a certain number. If, instead of six dice, we were to take seven, the critical number is, of course, so much further removed, viz: it would be necessary to throw oftener to get the seven three's, and so the number of casts increases with every additional cube, till we finally obtain enormous figures. But no matter how many dice, the three's must turn up, if we can throw them long enough, and if, in the case of 1,000 dice, it were to take 1,000,000 years, the three's must appear and reappear again and again after proportionate intervals.

Supposing now that, instead of dice, we were to take a glass filled with sand. There are, let us assume, 20,000 sand grains in the glass. Each particular grain occupies a certain position, which is bound to differ from that of all the rest of the sand grains; this the reader will doubtless admit. We shake the glass; the positions are altered, the order of arrangement is disturbed. We shake it again; the sand

grains are now in a totally different position. We continue shaking the glass and the time must come when each individual grain again occupies the exact position which it occupied when we originally started. It is a mathematical necessity, which all will admit who know anything of the calculus of permutations. The 20,000 sand grains may be looked upon as so many dice, which are bound to fall precisely as they once fell if we can throw them sufficiently often.

Now, I have strong grounds for assuming that my body is composed of atoms, or groups of atoms, of a limited number of elementary substances, or of one elementary substance, if all matter has been evolved from one primary element. The number of these atoms may be ever so great, it has nothing whatever to do with the inevitable result. I know also that all other bodies are composed of such atoms, or groups of atoms (molecules), not only those of the human species, animals and plants, but of inorganic substances, rocks, metals, fluids, gases; in short, of everything which exists in, upon or above the ground in the atmosphere. I know, furthermore, that the atoms of even the hardest and seemingly most enduring substances, such as agate and diamond, are in a state of continual vibration, that nothing can permanently retain its form, that the entire universe always has been, is now, and always will be, in a state of metamorphosis or continual change.

The time must arrive when the atoms or molecules which are now united in my body, after countless transformations and wanderings through all kinds of bodies, substances or intermediary stages, will once more unite in the same manner; in other words, the time must arrive when my life, like that of every other individual, will repeat itself. Yes, repeat itself, and not merely once, but an infinite number of times.

And more than this. If one of my readers should imagine that the atoms or molecules, which now constitute his body, are thus associated for the first time, I can only admire his simplicity. There is nothing new under the sun. Those molecules were united in this manner before, and

before this again, and 100,000,000 times previously, as far as our imagination can carry us back into the abysmal night of the æons of the past. In other words: each of my readers has been, ages ago, what he is now, has lived and gone through all this before, has felt and experienced what he now feels and experiences, down to the minutest details, has opened his *Journal of the Franklin Institute* billions of years ago and read the same lines; not once, but an endless number of times. The recollection, of course, is lost. Life and mind itself, consciousness or "soul" is only a product of matter, and if the same substances reunite in the same manner, the same phenomena must inevitably recur.

Let the molecules which now constitute my body undergo ever so many metamorphoses, let them even—which, of course, is very improbable—once fill a bung-hole, let them be scattered about in all manner of forms and conditions, in close contact or millions of miles apart; they must come together again, may the thought please or distress me; this is the iron logic of modern dynamics.

PHENOMENAL FRICTION.

By John H. Cooper.

When making experiments during the month of February, 1891, with the Thurston railroad testing machine, I noticed the ease with which the axle-box could be made to slide longitudinally upon the axle when the same was in motion.

The several boxes tried had about fourteen square inches of surface in contact with the axle; they were variously loaded, with weight from 262 pounds upwards, and the axle was running at speeds varying from 160 to 400 revolutions per minute.

One box could be moved by a pressure of one ounce when the axle was running, but required thirty-two pounds to move it when the axle was still.

Another box was moved by four ounces with motion, but required forty pounds without motion of the axle.

A third box under considerable pressure could be moved readily by a pull of six ounces, but fifty pounds would not start it when the axle was still, and indeed, on trial, all the muscular force I could apply to it by my hands, with my foot against the machine, failed to start it.

A spring balance was used in these experiments, for pulling the box in a line parallel to the axle.

Here we employ forces anywhere from 160 to one, up to perhaps 1,000 to one, for moving the same body, under the same load and conditions, except that of the revolving or standing shaft beneath it.

This phenomenon of friction proved a marvel to all who witnessed it. The temptation was great to theorize upon the extraordinary performance, but no theory was offered in explanation of it. A practical suggestion was made, however, in reference to planer-bed motions, and the like, which drag so heavily upon their fixed ways. If, as then proposed, revolving shafts were placed in the bed-ways, and the table fitted to them, a pound pressure would move the table and its load and forth on the revolving ways, where 1,000 pounds or more would be required to do this work upon the usual fixed V's of planers, as they are generally built.

Numerous applications of this principle will readily suggest themselves to the ingenious reader, when considering the necessity and the difficulties of moving dead loads, especially where ease and celerity of the movement may be required.

To the writer, this unique action, as if the loaded box were floating, was an instructive object-lesson in mechanics.

PROCEEDINGS

OF THE

CHEMICAL SECTION

OF THE

FRANKLIN INSTITUTE.

[*Stated meeting, held at the Institute, Tuesday, April 21, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, April 21, 1891.

Dr. WM. H. WAHL, President, in the chair.

Twenty-two members and three visitors were present.

Mr. Geo. M. Beringer was proposed for membership, and at the close of the meeting he was elected a member of the Section by the committee on admissions.

Prof. Joseph W. Richards read a highly interesting paper, entitled "On the Heats of Formation of Metallic Fluorides." It was referred for publication.

Mr. Reuben Haines followed with a paper, entitled "On the Ammonia Process in Water Analysis;" this was also referred for publication.

Mr. Haines presented another paper giving the results of an analysis of an artesian well water from Germantown; it was read by title and referred for publication.

Mr. Fred. E. Ives called the attention of the Section to a letter from Prof. Chas. F. Himes, discussing the experimental results and the theoretical explanation of Prof. Lippmann in the matter of photographing in colors. He also read a portion of a paper recently published by Capt. Abney, and bearing on the same subject.

Dr. Wahl, on behalf of Mr. John Carbutt, presented a communication on the "Diazotype Process of Photographic Dyeing and Printing." Several highly satisfactory specimens of results accomplished with this process were submitted for inspection by the members; they were regarded with much interest.

The Section then adjourned. WM. C. DAY, *Secretary.*

On LOCAL STANDARDS in WATER ANALYSIS with SPECIAL REFERENCE to PHILADELPHIA and its VICINITY.

By Reuben Haines.

[*Read at stated meeting of the Section, held February 17, 1891.*]

One of the difficulties presented in the "Analysis of potable water" is the obtaining of a suitable standard with which to compare the results of analysis of any given water, in order to form a correct judgment of its character for drinking purposes.

A general standard, such as is given in some published works on this subject, is found in many cases inapplicable or misleading. Special local standards are therefore advised for each locality. These may be obtained by collecting a considerable number of analyses of such well and spring waters of a given district as are, as far as possible, in a natural uncontaminated condition, and an average derived from these may form a standard of purity, to which a potable water should conform quite closely.

It is true, however, that one should not apply such a standard as a "hard and fast rule" to *all* waters in the particular locality for which the standard is devised. Different classes of waters should be judged by separate standards and the results of analyses of waters of different classes should be tabulated separately. Thus river waters should not be compared with well waters. They should be judged by somewhat different standards. Lake or pond waters may be judged by river-water standards under ordinary conditions of location and surrounding drainage area. Well waters, located within the same geological district, may be compared with each other. Yet the writer fully coincides with the statement of the late Prof. Wm. R. Nichols that "there always will be difficulty in deciding how near to any limit any suspicious water may come and still be used with a reasonable degree of safety;" and that

"every doubtful water must be considered by itself with all the light that can be brought to bear upon it."* Strictly speaking, a pure drinking water should be only one which is altogether above *any* suspicion of contamination with sewage or other injurious substances. But, it may not be practicable to obtain such absolutely pure natural water in some localities, as, for instance, in a village having no public supply. The question then arises, what constitutes a reasonably pure water under the special circumstances, and to what degree may the evidence of slight contamination through the occupations of man or beast be disregarded without risk of imperilling the health in any way, either at the present time or in the somewhat undefined future; for what may be safe now may not be quite safe a year hence. It is, therefore, highly desirable to become thoroughly acquainted with the locality, by personal inspection of the premises, and to confirm or modify the opinion derived from analysis by the facts observed. In many cases it is difficult, and in some impossible, to form any correct judgment at all without examination of the immediate vicinity of the well. It will not do, in all cases, to depend upon getting the desired information at second hand by correspondence or otherwise.

Nevertheless, a standard of some kind is desirable, in order to secure as much uniformity of practice as possible, and it will be found helpful to have special standards of the kind proposed.

In using such a standard it should be remembered, also, that the judgment is formed partly from the ratio one "element," so to speak, in the analysis bears to another, and partly from the consensus of all the facts, including some which may not be capable of being tabulated in the report.

Knowing of no published standard for well and spring water of the vicinity of Philadelphia, the writer ventures to give the results of some analyses which he has made, during the past fourteen years, of waters of this class, which he

* "Water Supply—Chemical and Sanitary," Wm. Ripley Nichols, p. 40–41.

believes both from analysis and from personal inspection of the premises, to be entirely pure and uncontaminated. He submits them as a contribution towards a definite standard for the vicinity of Philadelphia, on the Pennsylvania side of the Delaware River, with the hope that other chemists practically interested in this subject may likewise add the results of their experience, as the writer believes the aggregate number of analyses presented in this paper is too small to establish definitely the proposed standard for such a large area as these cover.

It should be further stated that in many of the following analyses other determinations were made, such as oxygen required to oxidize organic matter by potassium permanganate in place of sulphuric acid. As, however, at first, the Tidy form of this process and later the Kabel method, as modified by Leeds were used, the results of which processes are different and not comparable with each other, all results by the permanganate process have been omitted from the following tabular statement. It is desirable that in subsequent work this determination should be included.

The nitrogen as nitrates was in the earlier analyses determined by Williams' copper-zinc couple method, and in the latter ones by the phenol sulphonic acid method. I obtained excellent results in test analyses by the Williams method with solutions of potassium nitrate, provided the amount of the nitric acid in the solution was not very greatly in excess of the amounts usually found in highly contaminated drinking waters. In the presence of very large amounts of "free ammonia" in well waters, it is often almost impossible to get constant results, but in these cases the quantitative determination may safely be omitted as unnecessary for the purpose of sanitary judgment of the water; a qualitative test would, in that case, usually be quite sufficient. Yet in some cases much free ammonia may be accompanied by but little nitrate. The phenol sulphonic acid method was also tested against the Williams method and a known solution of nitrate, in my laboratory, and these two methods were found to give essentially identical results. There appears, however, according to Mr.

Bartram, to be some reason for questioning the accuracy of the phenol sulphonic acid method in presence of much chloride.

Neither of these methods, nor indeed, any colorimetric test is suitable for solutions containing comparatively very large percentage of nitric acid, such as organic liquids rich in nitrates.

In a number of the earlier analyses, a simple qualitative test for nitric acid was thought sufficient. The test used was the well known ferrous sulphate solution, with equal volumes of pure concentrated sulphuric acid and the water to be examined. This qualitative method was used by Prof. Nichols in his analytical work for the Massachusetts State Board of Health. Tests by the present writer with this solution showed that with careful manipulation it would indicate clearly about one part of nitrogen as nitric acid in 100,000 parts of water, and that it would show a faint reaction with 0·7 to 0·8 parts per 100,000. The test cannot be made a quantitative one, however, except in the sense above indicated. By concentration of the water by evaporation, one can secure a more distinct reaction, but it is very difficult at least, if not impossible, to get the depth of tint to follow exactly in accordance with the degree of concentration of the water, even when closely adhering to an identical mode of manipulation. The sulphuric acid used was always, of course, examined for nitric acid, and rejected if its presence was shown by this test.

Equal measured quantities of the acid and the water to be examined about 5 cc. of each were put into a test tube; the water first and the acid poured in gradually along the sides of the *inclined* test-tube so as to form a layer at the bottom without mixing much with the water. After cooling, a few drops of concentrated ferrous sulphate solution were added and the tube again plunged in cold water. If a colored ring formed at the junction of the acid and water, after slight agitation, the water was considered suspicious of contamination with sewage. If the ring of color was formed immediately and of strong tint, it indicated *dangerous* contamination, and the water was generally found to be

foul in respect to the other parts of the analysis. There are some cases, however, occurring occasionally in which the judgment rendered must be based *chiefly* upon the amount of nitrates present. It is, therefore, better to use a method which can be made quantitative.

In the following tables an *asterisk* (*) in the column of nitrogen as nitrates indicates that by the ferrous sulphate test either there was no reaction or only the faintest coloration after standing for about half an hour, showing less than one part of nitrogen as nitrate for 100,000 without any concentration of the sample.

The following are the tabulated results of analyses alluded to above, arranged according to the several districts included. All numerical results are stated in parts per 100,000.

GERMANTOWN.

No.	Description, Location, etc.	Date.	Free NH_3	Alt. NH_3	Cl.	Nitr'g'n in Nitrates	Total Solids.
1	Haines St E. of Chew St. Well, ...	1877	·0014	·0034	1·00	—	—
2	" " " "	1883	·0040	·0040	·80	*	—
3	" " " "	1886	none	·0036	1·80	·247	11·0
4	" " " Spring, . . .	1877	·0012	·0030	—	—	—
5	" " " same "	1884	·0012	·0034	1·00	*	—
6	" " " " "	1889	·0010	·0052	·80	—	10·5
7	" " " " "	1889	none	·0030	·80	*	—
8	High Street Well, ...	1877	·0026	·0064	1·40	*	5·0
9	Mill Street, E. of Willow, Well.	1880	·0022	·0062	1·80	*	22·8
10	Clapier Street Spring, ...	1889	·0010	·0052	1·60	·720	13·0
11	West Chelten Avenue Well, ...	1877	·0040	·0052	1·00	*	8·5
12	Wayne Junction Well, ...	1882	·0026	·0050	1·80	·520	—
	Average, ...	—	·0018	·0045	1·15	·495	11·8

VICINITY OF FRANKFORD.

13	Well, ...	1886	·0040	·0030	1·10	·741	10·5
14	Spring in Woods, ...	1888	none	·0030	.80	·257	9·0
	Average, ...	—	·0020	·0030	·95	·499	9·7

BRANCHTOWN TO CHELTEN HILLS.

No.	Description, Location, etc.	Date.	Free NH_3	Alt. NH_3	Cl.	Nitr'g'n in Nitrates	Total Solids.
15	York Road Well,	1890	none	·0040	1·20	·150	10·0
16	" "	1881	·0010	·0040	2·00	*	7·5
17	Washington Lane Well,	1886	·0010	·0050	1·00	·412	10·5
18	" " same Well,	1889	none	·0050	·80	·370	9·0
	Average,	—	·0005	·0045	1·25	·311	9·2

HAVERFORD AND BRYN MAWR.

19	Vicinity of Hav. College Well,	1880	·0010	·0056	1·10	*	12·9
20	" " "	1889	·0058	·0046	·70	·600	9·0
21	" " Spring,	1891	·0020	·0070	1·00	·750	11·5
22	Bryn Mawr Spring,	1880	·0022	·0070	·70	*	5·7
23	" same Spring,	1881	·0010	·0030	·70	*	5·0
24	" "	1883	·0026	·0066	·60	·263	4·5
25	" Well,	1880	·0040	·0050	1·10	*	10·0
26	" "	1883	·0010	·0038	1·20	·347	8·5
27	" "	1888	·0026	·0030	·70	·412	7·4
	Average,	—	·0023	·0051	·87	·474	8·3

OVERBROOK.

28	East of Penna. R. R. Well,	1882	·0046	·0040	1·45	*	12·0
29	West of " " Spring,	1887	·0040	·0030	·70	·249	10·5
	Average,	—	·0043	·0035	1·07	·249	11·2

SPRING MILL NEAR CONSHOHOCKEN.

30	Public Spring, hardness 12°·2,	1889	·0040	·0050	·50	·370	16·5

ARTESIAN WELLS.

31	Germantown, 312 feet deep, rock struck 8 ft. from surface,	1877	·0020	·0040	3·00	*	30·0
32	Columbia Ave. near Broad St.,	1886	·0160	·0030	1·50	·135	28·0
33	North Penn. R. R. near City,	1890	·0044	·0030	1·10	·100	15·0
34	Vicinity of Darby, Delaware Co., Pa.,	1889	·0010	·0050	1·10	·470	—

It is thought proper to place the above artesian wells in a separate class by themselves, and they are not therefore included in the following computation of averages for the above tabular statements. The artesian wells show plainly the degree of purity obtainable by them. Numbers 32, 33 and 34 are 100 feet in depth, or but slightly more than that. In No. 31 the water rises to about thirteen feet from the surface, and an iron tube extends nearly the whole distance through rather soft rock. Since the above analysis it has become impure, and is seldom used for steam purposes.

The following are the averages of the total figures in the above thirty analyses of shallow well waters, these being calculated only from the actual numerical results in each column, the qualitative tests as well as the blanks being omitted in the computation.

	Parts in 100,000.
Free ammonia,	·0020
Albuminoid ammonia,	·0045
Chlorine,	1·07
Nitrogen in nitrates,	·427
Total solids at 100° C.,	9·60

I ought to say that the figure for nitrogen as nitrates is not quite satisfactory, for it represents only one-half of the whole number of analyses, and assumes that the other half if determined quantitatively would yield about the same average which may not be strictly correct.

The determination of total solids is of not much importance, and is of use chiefly in this connection to show the general character of the water as regards hardness. It is to be noted that most of these well waters are quite soft. In hard sulphate waters the natural amount of chlorine appears to be higher (about double) than in softer waters. In the usual course of analysis the determination of total solids gives useful information in several ways.

In conclusion, the above average results is submitted as an approximate standard for the purest available shallow well waters of this district to be corrected by additional analyses.

As a necessary part of this work it is highly desirable to obtain figures representing the *highest limits* for waters of

this class under *specified conditions*. This will be more difficult to fix definitely, for a certain latitude must always be permitted to satisfy special conditions. This limit will certainly be different for different sections of the country. I think, however, that for the vicinity of Philadelphia on the Pennsylvania side of the Delaware River the figures for free and albuminoid ammonia will be found closely to approximate those given by Wanklyn in his book on water analysis. In certain parts of New Jersey, for instance, the limit is probably higher.

"On a SOURCE of ERROR in the DETERMINATION of NITRATES in WATER by the PHENOL SULPHONIC ACID METHOD."

By George H. Bartram.

[*Read at the stated meeting of the Chemical Section, March 17, 1891.*]

Owing to certain discrepancies in duplicate estimations of nitrates, by the phenol sulphonic acid process, I was requested by Dr. Samuel C. Hooker to make a series of experiments to ascertain under what conditions this process, which had hitherto been considered reliable, gave these varying results. The work was performed in Dr. Hooker's laboratory and under his direction.

As the principal result of these experiments it was found that phenol sulphonic acid undergoes a change shortly after preparation, as shown by the fact that chlorides, which have no effect on the determinations when made with freshly prepared phenol sulphonic acid, bring about low figures when the acid has been kept for some time before use.

The disturbing action of the chlorine is already well marked, when the quantity present in a given volume of water is about four times as great as the nitrogen of the nitrates. Correct figures were, however, obtained even when the phenol sulphonic acid had been kept eighteen months, with water in which the chlorine did not much exceed the

nitrogen of the nitrates. Water containing twenty parts chlorine to ·5 parts nitrogen gave the following decreasing quantities of N as the age of the solution increased:

N. present.	*N. found.*	*Age of phenol sulphonic acid.*
·5	·5	Freshly prepared.
·5	·5	1 day old.
·5	·45	2 days "
·5	·42	13 " "
·5	·40	17 " "
·5	·40	27 " "

Water containing ten parts chlorine to ·5 parts nitrogen gave a similar series of figures:

N. present.	*N. found.*	*Age of phenol sulphonic acid.*
·5	·5	Freshly prepared.
·5	·5	2 days.
·5	·42	13 "
·5	·40	17 "

With water containing two parts chlorine to ·5 part nitrogen the results similarly fell off.

N. present.	*N. found.*	*Age of phenol sulphonic acid.*
·5	·50	Freshly prepared.
·5	·50	13 days.
·5	·40	27 "

The above determinations were made in duplicate and the standards were always freshly prepared.

A solution of phenol sulphonic acid two and a half months old gave, as the mean of four determinations, ·30 instead of ·5 N, in the water containing ten parts chlorine to ·5 N.

A solution eighteen months old also gave ·30 N for the same water.

By the removal of the chlorine correct results were obtained.

The above experiments show that in the presence of relatively large quantities of chlorine twenty per cent of the nitrates, or in extreme cases even as much as forty per cent. may fail to be recorded by the use of this process, unless the precaution be taken to use freshly prepared phenol sulphonic acid, or to remove the chlorine.

ANALYSIS OF A CHROMITE.

BY H. PEMBERTON, JR.

[*Read at the meeting of the Chemical Section, held March 17, 1891.*]

On the Pacific Coast of the United States there are several deposits of chromite. As no complete analysis of any of these minerals has, to my knowledge, been published, it may be of interest to place on record an analysis I made some time ago.

According to Wm. Irelan, Jr., State Mineralogist of California, throughout the Santa Lucia Mountains and the coast hills of San Luis Obispo County, Cal., are found serpentine rocks with beds of chromite, in greater or less masses existing as loose and fragmentary rocks in the ravines and on the hillsides, and as pockets and veins on the mountains. One of the best mines of the neighborhood is the Pick and Shovel, located on the South Fork of Chorro Creek, at an elevation of 1,800 feet.

A sample of the chromite from this mine, selected as free from gangue as possible, gave on analysis the following results:

Cr_2O_3,	52·68
Al_2O_3,	11·40
Fe_2O_3,	3·52
MgO,	16·23
FeO,	11·77
MnO,	0·15
SiO_2,	3·40
H_2O,	0·94
	100·09

An analysis of the gangue showed it to be serpentine, containing a trace only of ferrous oxide. No other foreign mineral could be detected on a careful examination. Deducting, therefore, the water, silica, and quantity of magnesia (3·26 per cent.) combined with the silica (as serpentine)

and calculating the remainder to 100 parts, we obtain for the pure mineral:

Cr_2O_3,	56·96	·374	
Al_2O_3,	12·32	·121	= ·519
Fe_2O_3,	3·81	·024	
MgO,	14·02	·350	
FeO,	12·73	·177	= ·529
MnO,	0·16	·002	
	100·00		

$R_2O_3 : RO = 519 : 529 = 1·00 : 1·02$.

The mineral was decomposed by fusion with sodium carbonate. (Christomanos' process slightly modified.) The mixture of ore and Na_2CO_3 is heated over night by a Bunsen burner, the crucible being loosely covered by its lid. Next morning it is heated for one hour over the blast lamp, with frequent stirring. The process has the advantage of giving complete decomposition of the mineral, with the introduction of no bases other than the alkalies.

The ferrous oxide was determined by solution in sulphuric acid in a closed tube, under pressure. According to Francis C. Phillips (*Fres. Zeitschrift*, **12**, 189) 1·34 is the specific gravity of the acid that is most favorable to the solution of chromite in this manner.

An Apparatus for Heating Substances in Glass Tubes under Pressure.

By H. Pemberton, Jr.

[*Read at the meeting of the Chemical Section, held March 17, 1891.*]

Chemists who do not happen to have in their laboratories oil or air baths for heating closed tubes, can make an air bath at short notice, from materials furnished by all dealers in steam fittings.

Order:

(1) One four-inch wrought-iron pipe, eighteen inches out to out, with usual thread on each end. At about nine

inches from either end this pipe is drilled and tapped for a one-inch nipple, in such a manner that a pipe introduced would pass, not on a line with the radius, but about halfway between the axis of the four-inch pipe and its walls; in other words, it would be on a line with a chord of the circle.

(2) One one-inch wrought-iron nipple, two inches long, one-inch thread on one end.

(3) Two four-inch malleable iron caps, drilled and tapped for a one-inch pipe.

(4) One one-inch wrought-iron pipe, twenty-four inches out to out, with a three-inch straight thread on each end.

(5) Two one-inch iron caps. A hole, one-eighth of an inch in diameter, is drilled in the end of one of these caps.

The above order can be given *literatim*, and will be understood by the dealer, who will furnish, at a trifling cost, the materials, cut and tapped as ordered.

Fig. 1 shows how the whole is put together. The numbers on the figure correspond also to the numbers of the paragraphs of the order as given above.

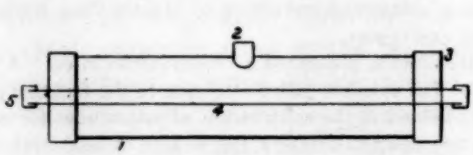


FIG. 1.

FIG 2.

Fig. 2 is an end section. A cork is inserted in 2 and through it a thermometer, the bulb of which is on a level with the interior pipe. The whole is supported on a few bricks at either end, and is kept steady and in place by a couple of weights or half bricks. It is heated by one or two Bunsen burners, according to the temperature desired.

PROCEEDINGS

OF THE

ELECTRICAL SECTION,

OF THE

FRANKLIN INSTITUTE.

[*Proceedings of the stated meeting of the Electrical Section, held Tuesday, April 7, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, April 7, 1891.

Prof. EDWIN J. HOUSTON, President, in the chair.

Present, thirty-four members and visitors.

The minutes of the previous meeting were read and approved.

The treasurer presented bills for stationery, printing and clerical work, which were ordered paid when approved.

Twelve nominations to membership were referred to the committee on admissions.

At the request of the Secretary of the Institute, the subject of re-wiring the building in accordance with best modern practice, with a suggestion that detailed specifications, be prepared by the Section for that purpose, was presented and referred to a committee consisting of Messrs. Pike, Billberg and C. Hering, to consider and report.

Prof. C. Hanford Henderson presented a communication on "A Suggestion in Arc Lighting." The speaker called attention to the fact that progress in illumination has consisted in the substitution of continuous for reservoir systems. While the two essential factors, the source of heat and the substance to be made luminous, are combined in the candle, oil and gas as ordinarily used, they are separated in incandescent gas and electric lighting. In arc lighting the supply of energy is continuous, but the light-yielding substance, carbon, must be frequently replenished. The question as to whether arc lighting could be made continuous had therefore presented itself, and an experimental attempt was made to use gas issuing from hollow wrought-iron terminals, in place of the solid carbon. It was found that the gas and the arc formed two mutually repellent crescents of light; the terminals were fused and one of the gas jets was closed up. This negative result not being thought conclusive, the suggestion was brought before the Section in the hope of discussing the question as to whether the problem was sufficiently promising to merit further attention.

Prof. Houston stated in discussion that hollow carbon electrodes had been used at different times since 1852, fed by hydrocarbon oil, and by gas containing pulverized carbon, and the like, but without success, owing to the fusion of the terminals. As the intensity of the light varies with the sixth

power of the temperature, he considered a continuous arc impossible with our present refractory materials. There was further discussion by Messrs. Pike, C. Hering and Wahl.

Prof. L. F. Rondinella read a paper on "An Experimental Analogue for Direction of Induced Currents," which was discussed and referred for publication.

Prof. H. S. Hering gave the result of continued experiment on "The Effect of Atmospheric Pressure on Batteries," which seemed to show that the E. M. F. of standard cells did not become constant until about two hours after setting up. This result elicited considerable discussion.

Upon motion of Mr. C. Hering, the treasurer was instructed to purchase a letter-box for the reception of queries on electrical subjects, which would be periodically submitted to the Section.

The meeting then adjourned. L. F. RONDINELLA, *Secretary.*

AN EXPERIMENTAL ANALOGUE FOR DIRECTION OF INDUCED CURRENTS.

BY L. F. RONDINELLA, M.E.
Prof. of Drawing, Central Manual Training School.

[*Read at the stated meeting of the Electrical Section, April 7, 1891.*]

The striking analogies which exist between current electricity and magnetism, form an important aid in acquiring a clear understanding of the fundamental principles of modern electrical science. For example, an electric current is assumed to pass through a generator from its south to its north pole, and from thence through an external circuit back to the south pole again. So, also, are magnetic "currents," or lines of force assumed to pass through and outside of a magnet.

In an induced electric current lines of force are made to flow around a conductor in a right-handed or clockwise direction when the observer is looking along the conductor in the direction of the current, or toward the south pole of the generator. Similarly, in an induced- or electro-magnet, an electric current is made to flow around the core of the magnet right-handed when the observer is looking in the direction of the lines of force in the core, or toward the south pole of the magnet.

The directions of lines of force are usually discovered by

the effect produced upon a small permanent magnet or magnetic needle, which will always come to rest with it, north pole pointing in the direction in which they flow through it. Then since the direction of a current depends upon the direction in which the lines of force flow round it, the former also is discovered by noticing the way in which the needle is deflected by the latter.

As there are two directions in which the current may flow through the conductor, and two positions in which the needle may be placed relative to it (above or below), it has been found necessary to use some *memoria technica* to quickly

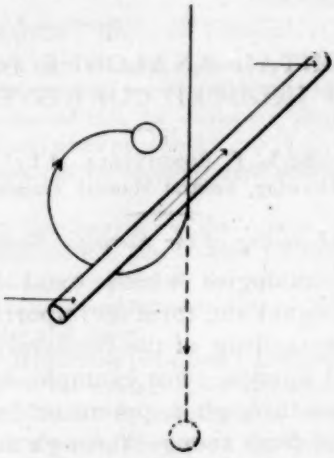


FIG. 1.

determine the direction of the current, from the way in which it deflects a needle; and among several devices that have been proposed, the following are most used, although they all present certain difficulties.

Ampère's rule is to imagine an observer swimming with the current in a conductor, with his face always toward the north pole of the needle, which will then be deflected toward his left hand. The disadvantages of this rule are the difficulty of imagining the direction of the observer's left hand when he is placed in unusual positions, and the fact that it was intended for determining the deflection of the needle from the direction of the current which it pre-

supposes to be known, and it must therefore be used inversely for our purpose.

Another device is to remember the fact that when a current flows from South to North Over a needle, its north pole points West, by noticing that the direction initials spell the word SNOW; and similarly a third rule impresses the statement that when a current flows from North Over a needle to South its north pole point East, by spelling the word NOSE with the direction initials. A vital point that seems to be most troublesome to remember in these, is whether the current is over the needle or *vice-versa*, and the difficulty of always finding a part of the conductor running north and south interferes with their direct application.

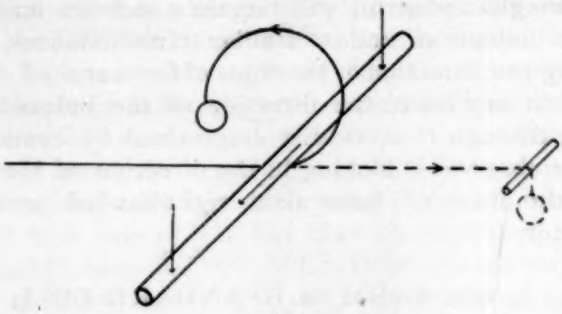


FIG. 2.

None of these devices can be used to predict the direction of the current that will be induced in a conductor when it is moved in a certain direction through a known magnetic field, and for this purpose the only one familiar to the writer is to use the right hand with (*a*) the thumb, (*b*) the first and (*c*) the second finger pointed outward in the three rectangular directions to represent respectively (*a*) the direction in which the conductor is moved, (*b*) the direction of the lines of force, and (*c*) the direction of the induced current. The trouble in applying this is to remember which hand to use and what each finger represents.

As an addition to these, and as an analogy that is not liable to confusion, the following experiment is suggested: If a small weight be suspended at the end of a piece of string (*Fig.* 1), the force of the weight acts downward

in the direction of the string, and the latter may be used to represent a "line of force" acting vertically downward. If then a horizontal rod, which may be used to represent a "conductor," be moved from left to right so as to strike sharply against the string a short distance above the weight, the latter with its "line of force" will revolve around the rod or "conductor" right-handed—a correct analogy to the phenomenon of electric induction. To represent lines of force acting horizontally in either direction, or vertically upward, the string may be held opposite or below a rod over which it is passed (*Fig. 2*), the force of the weight on its end acting along it in the proper direction. The rod may then be moved as desired, and the resulting revolution of the weight and string will remain a correct analogy for electric induction under similar circumstances. Then knowing the direction of the lines of force around the conductor in any case, the direction of the induced current flowing through it is readily determined by remembering that the observer is looking in the direction of the current when the lines of force flow right-handed around the conductor.

A NEW FORM OF STANDARD CELL.

BY CARL HERING.

[*Read at the stated meeting of the Electrical Section, held Feb. 24, 1891.*]

The standard cells used at present may be said to be limited to the Clark cell and the Daniell cell. Each one of these has its specific advantages and disadvantages. The Clark cell is always ready for use, but is difficult to make, is affected by temperature, and has the important disadvantage that if by accident it is short-circuited it polarizes so rapidly and so much, that it is of no use at all until it has been allowed to rest and recuperate for some time. Such accidental short circuits are not unlikely to occur, and they may furthermore occur, by the crossing of two wires, for instance, without the knowledge of the operator, thus making it possible that the

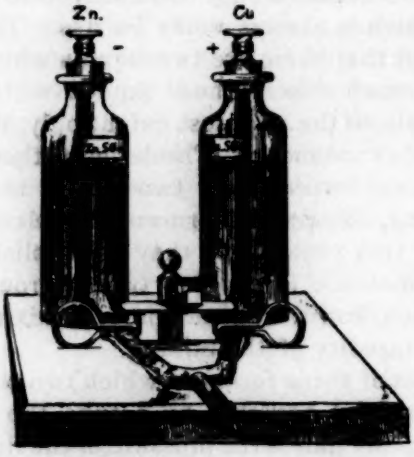
measurements made with it are incorrect. The Daniell cell, on the other hand, does not polarize at all, it may be short-circuited and used almost immediately afterwards; furthermore, it is very easy for any one to construct a Daniell, and it is practically independent of temperature. But it has the important disadvantage that it must be freshly set up just before using and cannot be kept standing. In their good and bad qualities, therefore, the two cells are diametrically different.

The object of the present form of cell is to try to combine all these good points in one and the same cell. As the polarization of the Clark cell is an inherent fault and cannot be remedied, there remains only to endeavor to devise a form of Daniell which is always ready for use. The difficulty is due to the fact that there are two liquids which must be in contact with each other; these liquids will tend to mix, and if they mix to the slightest extent only, the cell can no longer be used as a standard. The difficulty therefore resolves itself to a means for enabling two liquids to be in contact without mixing. Numerous forms of Daniell cells have been devised with this object, but they accomplish it only in a degree, as for instance, by capillary tubes, porous diaphragms, etc., all of which, however, do not prevent mixing, but merely diminish the rapidity of the mixing.

In a number of these forms, in which two bottles or reservoirs are connected together by a tube, the mixing takes place quite rapidly unless the pressure of the liquid is exactly the same in both bottles, a condition not always easily complied with.

Some time ago the writer had occasion to use a cell of this form devised by Prof. Barker, which consisted of two bottles containing the two liquids with their respective electrodes, the bottles being connected at their bottom by a small tube with a stopcock. This cock was opened only during the moment when the cell was being used. But even during a few seconds no small amount of mixing took place, especially if the pressure was different in the two bottles. It is very difficult to avoid the latter, owing to the different specific gravities of the liquids and their different

heights in the two bottles. The writer therefore modified this form by inserting two stopcocks in this tube and allowing the portion of the tube between the two cocks to communicate directly with the air, as shown in the adjoining cut. By this means the liquids, which mix at their contact, are allowed to drain off as rapidly as they mix. As there is an outward flow of the liquids through the cocks, the mixed liquids cannot pass back into the bottles. The liquids in contact with the electrodes will therefore always remain pure. To prevent too rapid a flow, which would needlessly waste the liquids, some filter paper, cotton or asbestos is packed into the tubes. The intermediate tube may be



drained completely, there is always a film of liquid sufficient to answer for a contact if the cell is used on open circuit, which is the only way any standard cell should be used. It was even found that the film of liquid through the cocks when closed, was sufficient to give an electro-motive force which, however, falls as soon as the slightest current is allowed to flow.

The internal resistance of such a cell is naturally quite high, from 10,000 to 20,000 ohms, but this is no disadvantage for the usual class of open-circuit testing.

The e. m. f. of a Daniell cell, according to a report of Dr. Fleming, is as follows: when the specific gravity of the zinc

solution is 1·2, and that of the copper is 1·2, when the zinc is amalgamated, and the copper is freshly electroplated with an untarnished, salmon-colored coating, having no brown spots, the e. m. f. is 1·105 true volts at about 16° C. The temperature correction is negligable. The solutions and metals must be perfectly pure, and the solutions made with distilled water and filtered, being diluted to the required amount after filtration.

Such a cell may be kept standing indefinitely, and is always ready for use. The only remaining objection is that the copper electrode does not remain clean. It should be freshly electroplated, before using, which is readily done in a test tube with a platinum anode. To overcome this, the writer suggests to amalgamate the copper, and when not in use to keep it in a tube of mercury. Whether this introduces other objections has not yet been tested.

Another modification suggested is to place in the bottom of the bottle containing the zinc solution, a quantity of pure zinc scrap, not in contact with the electrode. Similarly, copper scrap in the other bottle. Should any mixture of the liquids take place, they will be freed of their impurities by these metals, which, not being in contact with the electrodes, will not affect the e. m. f.

In experimenting with this cell a curious phenomenon was noticed which does not appear to be known. It was found that the slight pressure produced in the bottles by inserting the corks at the top, produced decided differences in the e. m. f. of the cell; this was noticed for each of the liquids, and was different in the two cases. It was therefore found essential to use a cork with a hole through it, so as to prevent this pressure from being exerted. The effect of this pressure has since been investigated by a very extended and interesting set of experiments made by Prof. Hermann Hering, and will form the subject of a paper to be read by him following this one.

BOOK NOTICES.

The Theory and Practice of Surveying.—Designed for the use of surveyors and engineers generally, but especially for the use of students in engineering. By J. B. Johnson, C.E., Professor of Civil Engineering, in Washington University, St. Louis, Mo., etc. Seventh revised and enlarged edition. 730 pp. New York: John Wiley & Sons.

This work, now so well known, the first edition of which was favorably noticed in this journal so late as January, 1887, has already reached its seventh edition, and now presents some noteworthy additions and other modifications.

A new table of magnetic declinations in North America, published by the Coast Survey in 1890, and a new table for refraction-corrections to be applied to the sun's declination in using the solar compass, are given in place of the former ones; and some new surveying instruments are described and illustrated, including the cross-section polar protractor used in the tunnel for the new Croton aqueduct.

The opening sections of Chapter vii, on Land Surveying, have been re-written, and in doing this greater prominence has been given to the United States public lands' system. An important addition is a series of rules deduced from legal decisions bearing on the re-survey of private lands.

Chapter viii, on Topographical Surveying, is substantially unchanged, except by the addition of a description of Porro's telescope, which reads stadia distances from the vertical axis of the instrument.

A new appendix (E) gives the positions of the base-lines and principal meridians used in laying out the United States public lands; and a new table (xii), condensed from the 1890 *Manual of Instructions*, issued by the General Land Office, gives the necessary data for obtaining the azimuth of *Polaris* at any hour, thus obviating the necessity of waiting for an elongation of the star.

We are glad to note that the very serious lack of an index has been, in a measure, supplied. A very much more complete one is still a great desideratum.

T.

First Lessons in Metal-Working. By Alfred G. Compton. pp. vi, 170. New York: John Wiley & Sons. 1890.

The author has prepared this work as a companion volume to "First Lessons in Wood-working," and in it explains the methods of doing forge work, foundry, vise- and other purely hand-work. About one-third of the book is devoted to exercises on the forge, telling in detail the methods to be followed to produce the pieces required. About an equal amount is devoted to the production and properties of cast iron, wrought iron and steel. Foundry work is scantily treated of in seven pages and almost as many erroneous ideas can be gathered for this part as there are pages. In the portion devoted to chipping and filing, the instruction is defective, in that the part

played by the hands and arm of the workman are forgotten. The rest of the book is given to drilling, hack-sawing and soldering.

A judicious selection of American names, instead of foreign ones, would have taken away much of the flavor of having been worked up from text books instead of actual practice, and perhaps would have given the students the idea that, for instance, files are made in this country and that iron is made in a few places in the United States.

Covering so much ground in a few pages particular care should have been taken that the statements made and the direction given were exactly correct, but there is in some parts of the work a looseness of statement that is sure to convey incorrect ideas.

The author has in many cases considered that one view (not isometric) gives sufficient information as to the shape of the piece. This is unfortunate, as in no other places are correct working drawings of so much use as in teaching elementary work of this character, and working to drawings properly made adds nothing to the labor of making a piece, but gives the student a better idea of what working drawings are for than can be obtained in any other way. H. W. S.

Constructive Steam Engineering. By J. Jay M. Whitham, M.E.; C.E. pp. v, 900. New York: John Wiley & Sons. 1891.

The author in this volume has covered the subject of steam engineering, as far as the appliances go, more completely than has ever before been attempted. There is hardly a single branch of the subject, of which numerous examples are not given. While, of necessity, trade circulars have been the principal sources of his figures, the grouping together of all the information obtainable on any one subject will be of great convenience to those desiring extended knowledge of what appliances are obtainable, without having to refer to innumerable pamphlets.

Something of the theory is added, which, however, adds little to the value of the work, as those to whom the work will be of most service have probably all that they desire in that line.

It is well printed, well indexed, and altogether is a desirable work to have. H. W. S.

Méchanique Frofessionelle, Outils, Organes, Machines Élémentaires. By Ch. Casalonga. Part 1. Tools. Office of the *Chronique Industrielle*. pp 103 and 10 plates. Paris. 1890.

This work is the first part of a series of ten, each of which is to be devoted to one branch or specialty of construction. There are three general divisions in this volume, the first devoted to the metals employed in construction, the second to hand tools, and the third to some of the machine tools.

That portion relating to the metals employed gives generally the physical characteristics of wrought and cast iron, steel, copper, bronze or brass, and phosphor-bronze. This is followed by a series of notes on all sorts of sub-

jects relating to the metals:—as homogeneous iron, prevention of corrosion, prevention of crystallization, bronzing and leading iron, preservation by painting with various substances, case-hardening, removing rust, forging steel, manganese and chrome steel, etc., testing machines, manganese bronze, delta metal, aluminium bronze, etc. This is followed by a bibliography, covering references to French journals for the past forty years, and this is followed by the advertisements of the dealers in the various metals and current prices for February, 1889. Under the head of hand tools are files, chisels, hammers, drills, reamers, taps and dies, milling cutters, wrenches and handles. This part is also followed by numerous notes such as recutting files by sand jet, Elliott's cutting-off tool, Stephen's vises, etc.; which is again followed by bibliography and advertisements and price-list. The portion relating to machine tools is divided into such as are used for iron, including drilling, boring, planing, shaping and milling machinery; machine tools for wood, covering sawing and planing machines, and machine tools for forging. Among the notes, radial drills, portable drills, the "Stow" flexible shafting, emery wheels, multiple drills, special tools for facing nuts, band saws, etc., are considered. This part also ends with a bibliography, advertisements and current rates in October, 1889. The plates give tables of dimensions for taps, tap wrenches, end, box, pin and S wrenches, adjustable pieces for turning an ordinary face plate into a chuck and sockets for the drill press, all, of course, in centimetres. H. W. S.

The Mechanics of Engineering and of Machinery. By Dr. Julius Weisbach, vol. 3, part 1, section ii. "The Machinery of Transmission and Governors," revised and enlarged by Gustav Herrmann; translated by J. F. Klein, D. E. 8vo, pp. vi, and from 535 to 1084. New York: John Wiley & Sons. 1890.

Of the many works on the machinery of transmission, but few are as yet available to the English-speaking engineer. The greater part of the literature on this subject is written in the German and French languages and of this but little has been translated into English. This volume will, undoubtedly, prove of great value to the American engineer and more especially to the engineering student, being a complete theoretical investigation into this subject. The mathematical methods used in the earlier volumes of this work, which were of a most indirect character and which have to a great extent caused vol. 1, on the "Strength of Materials," "Hydraulics," etc., to be almost entirely replaced by other text books in our technical colleges, are abandoned in this volume, the methods of calculus being used where the deductions are thereby facilitated. On the other hand, the work is hardly one which could readily be used by the practical man, since the results are not arranged in such a way as to be easily accessible. For this purpose, *Der Konstrukteur*, which at present is being published in English in the columns of *Mechanics*, will be preferable. For those, however, who wish to acquire a theoretical knowledge of the machinery of transmission, the book may be highly recommended. The first chapter, which is on "Flexible Trans-

mitters," contains a discussion on the considerations which enter into the manufacture of hemp and wire ropes, chains, hooks and other accessories. Formulæ showing the relative value of these organs for lifting are deduced, the transmisson of power by ropes and chains having been considered in a previous volume. In connection with this subject are given a few tables, showing the relative strength, weight and cost of different ropes and chains, from which it is evident that in all cases wire rope is the lightest and most economical.

Screws and screw wheels are next discussed. The functions of screws and the advantages of different types for different purposes are clearly set forth. The forces required to produce others and to overcome the frictional resistances and the corresponding efficiencies are established for different forms of threads. A few tables giving the proportions of threads for different systems would not be out of place here. Considerable space is devoted to crank trains. Graphical methods of finding the motions and velocities in different parts of these mechanisms are clearly explained. There are also numerous descriptions and illustrations of various mechanisms of this class, and their practical applications are given. Under this head, the action of the weight and velocity of the reciprocating parts of the steam engine has also received a thorough discussion.

Chapters vii and viii on Cam Trains, Engaging and Disengaging Gears, give a description of the various forms of clutches, ratchet motions, escapements, etc., and of the theories underlying the same, with numerous illustrations of their practical application.

The last chapter is on Regulators, and treats in this connection of the various forms of brakes, the fly-wheels and governors, with analytical and graphical explanations of their action. Finally, there has been added by Prof. Herrmann an appendix on the graphical statics of mechanisms. In this the supporting surfaces of mechanisms are replaced by a reacting force, which would act in a certain direction were there no frictional resistances. For the latter a correction is made in the direction of the reacting force. This principle is applied to a number of cases occurring in practice, and proves to be a simple and efficient method of determining the forces in mechanisms. E. R. K.

Cosmic Law of Thermal Repulsion. An essay suggested by the projection of a comet's tail. New York: J. Wiley & Sons. 1889.

In reading that recent anonymous little essay, on "The Cosmic Law of Thermal Repulsion," one is especially struck with the novelty of the idea on the one side together with a desire to base all argument and reasoning upon the most recent conventions of science, while one of the most fundamental notions of modern physics has been entirely overlooked.

From this point of view the book seems to be somewhat unbalanced. Whether this condition of things exists because the point of support does not bear the necessary equilibrium relation to the centre of gravity, or whether this latter point has been lost sight of in the "conjugal antagonism of heat

and gravity" might be a subject for further debate. In Section 2 on "General Principles," the writer seems to have ignored entirely the sharp distinction which is now insisted upon in science between force and energy. While a correct definition of force is implied at the outset, almost immediately several forms of energy are placed in the list of forces; *e. g.*, "gravitation and heat" are mentioned in the same clause as ever-existing *forces* when matter is present, while in the next sentence the *force exerted by heat* is referred to. It seems unfortunate then that in a paper of this sort the now universally accepted classification of nature into *matter* and *energy* should not have been adopted.

We have in science the most important generalizations which are called the laws of conservation in the case of both matter and energy, and the truth of which is necessarily assumed throughout the essay, but who ever speaks now of the law of the "conservation of force," unless one reads it in some antiquated text book? Again, on the top of the eighth page is found the statement that expansion "is proportional (with certain well-known exceptions) to the amount of heat absorbed by the body." Now this is not only in general not the truth, but in no single known instance is it true; in other words, the exceptions embrace every known case. The reason is obvious. If heat be applied to a rod for example, it is expended in doing two things, viz: expanding the rod against external forces, *i. e.*, doing external work, and in raising the temperature of the body; but this rise in temperature, and therefore the part of the heat applied required for this purpose, is dependent upon the specific heat of the substance which always varies with the temperature.

It would seem that at the beginning of Section 9 would be a suitable place for inserting the clause "(with certain well-known exceptions)" as the expansion is really equal in all directions only in isotropic substances, which are comparatively few.

On the whole, however, it should be said in fairness that the essay seems extremely well written and to show much thought in its preparation, as also a wide acquaintance with the works of many well-known writers.

The interest is held throughout by many amusing illustrations, of which the little fish story of Section 4 ought especially to be noticed. The ideas of "thermal repulsion," however, as well as the general conclusions arrived at, are so novel, that it is to be feared they will not immediately take a place among the generally accepted theories of science, but as to what may ultimately be the result of such speculations, we would not like to venture an opinion.

A. W. G.

Formulæ and Tables Used in the Application of the Method of Least Squares. By Malverd A. Howe. Terre Haute: Moore & Langen. 1890.

Of the thirty-two pages of this little book, nineteen are devoted to the formulæ of the theory of probabilities and of the method of least squares, the rest to a set of tables giving the values of the different expressions used in the applications of this theory. It forms altogether a very convenient book of reference for those whose work requires the use of these important methods. The formulæ are clearly and distinctly arranged and can readily

be used even by those who have little or no knowledge of the theory of the subject.

The tables are eleven in number:

I gives

$$\frac{2}{\sqrt{\pi}} \int_0^{h\,x} \frac{d\,(h\,x)}{e^{h^2 x^2}}$$

to three decimal places, for the argument $h\,x$.

II gives the same quantity to seven decimal places.

III gives

$$\frac{2}{\sqrt{\pi}} \int_0^{0.47694\,\frac{x}{\gamma}} \frac{d\,(h\,x)}{e^{h^2 x^2}}$$

to five decimal places for the argument $\frac{x}{\gamma}$

IV and V give $0.6745\,\frac{1}{\sqrt{n-1}}$ and $0.6745\,\frac{1}{\sqrt{n\,(n-1)}}$

respectively, to 4 decimal places.

VI and VII give $0.8453\,\frac{1}{\sqrt{n\,(n-1)}}$ and $0.8453\,\frac{1}{n\sqrt{n-1}}$

respectively, to four decimal places.

VIII and IX give the value of a^2 for $q = 1$ and $q = 2$, respectively, for use in "Pierce's Criterion," for the arguments m and n.

X gives $n \times 0.675$ and XI gives

$$\frac{1}{e^{\frac{1}{2}}}$$

Logarithmic Tables. By George W. Jones. Ithaca: Author. 1891.

This collection of tables consists of seven parts: I, A table of six place logarithms of numbers; II, Physical and mathematical constants; III, Sines and tangents of small angles; IV, Trigonometric functions, both natural and logarithmic; V, Napierian logarithms of numbers from 1 to 1079; VI, Meridional parts; VII, Addition—substraction logarithms.

The typography and the arrangement of the page, which are points of the greatest importance in a book of this kind, are both excellent. We would call attention particularly to Tables II, V and VII, as being very valuable additions to the other more frequently used tables. The collection of constants in Table II is unusually complete. In Table IV the natural and logarithmic functions are arranged in parallel columns. The *sin.*, *cos.*, *tan.* and *cot.* are given for every minute of arc from 0° to 180°. The principal

defect in the tables is found here, in the absence of tables of proportional parts for making the corrections for seconds.

Appended to the tables is a short description of the mode of using them.

Treatise on Trigonometry. By Oliver, Wait and Jones. Ithaca. 1890.

The most striking features of this text book are the use of the methods of projection in the proofs of the principal formulæ and the addition of much matter, mainly in the way of applications to surveying, navigation and astronomy, not generally found in such text books.

The subject is introduced by some paragraphs upon the method of coördinates in determining the position of a point in a plane, and the principles of directed lines and projections. Then follow the methods for the measurement of angular magnitude and the definitions of the trigonometric functions for the general angle. These and the other principles throughout the book are illustrated by numerous exercises and examples. The different parts of the subject are then discussed in the usual order, the general formulæ and the solution of triangles, right and oblique, followed by the formulæ of spherical trigonometry and the solution of spherical triangles.

The development of the subject from the principles laid down at the beginning is clear and logical, and we certainly have no fault to find with it on that ground. We may be allowed to express a doubt, however, whether the general plan here adopted is the best form in which to present the subject to the mind of the average beginner.

Besides the portions ordinarily given in a beginner's course, sections are given upon the functions of multiple angles, inscribed, escribed and circumscribed circles, areas and projections of polygons, the derivatives of the trigonometric functions, their development in series and methods of computation, the relations between plane and spherical triangles, and the applications of the subject noted above. These last are very complete in the matter of problems, and furnish excellent work for drill purposes.

The general appearance of the book is excellent, the typography clear, and the figures well drawn and distinct.

Photographic Mosaics By E. L. Wilson, 833 Broadway, New York, 1891. Price, 50 cents; cloth, $1.

Photographic mosaics, the oldest of the American photographic annuals, has been enlarged to double its former size, and contains 284 pages of matter of interest and value to photographers, besides 12 full-page process illustrations. Of its 284 pages, 112 are devoted to a retrospect of last year's work, and the remaining 172 pages consist of original communications. I.

Mr. ROBERT GRIMSHAW, General Editor of the Trades Department of *Funk & Wagnall's Standard Dictionary*, being desirous of making as complete as possible his list of mechanical and industrial terms, requests manufacturers of machinery and tools having important parts not found on those of other makers, or the names of which are not yet in general use, to send the name, definition, and use of each such part to him, at 115 Bible House, New York City.

GIFTS TO THE LIBRARY OF THE FRANKLIN INSTITUTE.

Adams, H. C. Gas Coals in the United States. From the author.
Agricultural Experiment Station of the University of Wisconsin. Seventh annual report. From the station.
Aid to Astronomical Research. No. 2. From Prof. E. C. Pickering.
Alabama Agricultural Experiment Station. Bulletin No. 20. From the director.
American Ephemeris and Nautical Almanac for 1893. From the superintendent of nautical almanac.
American Society of Mechanical Engineers. Transactions. Vol. 11. From the society.
Baker, Henry B. Malaria and the Causation of Intermittent Fever. From the author.
Brooklyn Library. Bulletin No. 28. From the library.
Connecticut Board of Agriculture. Twenty-third annual report of the secretary. From the state librarian.
Connecticut Public Documents. 1890. Vols. 1 and 2. From the state librarian.
Delaware College Agricultural Experiment Station:
Bulletin No. 4. From Joseph M. Wilson.
Bulletin No. 10. From the station.
Engineers' Society of Western Pennsylvania. Proceedings of meetings of September and October, 1890. From the secretary.
Foote, Allen R. Dangers to Firemen from Electric-light Wires in Buildings. From the author.
Germanischer Lloyd. Internationales Register. 7ter Nachtrag, 1890. From L. Westergaard & Co.
Grand Trunk Railway of Canada. Report for half-year ended June 30, 1890. From Sir Joseph Hickson.
Harvard College Observatory. Annals. Vol. 21. Part 2. Vol. 24. Vol. 30. Part 1.
History of Harvard College Observatory. From the director.
Illinois State Laboratory of Natural History. Articles 5 to 10. From the director.
International American Conference. Minutes. From the executive officer.
Manchester Public Free Libraries. Thirty-eighth annual report to the council of the city. From the librarian.
Massachusetts State Agricultural Experiment Station. Analyses of commercial fertilizers. From the station.
Massachusetts Institute of Technology. Commemorative address by Augustus Lowell, Esq. From the institute.

ANNUAL REPORT OF THE DIRECTOR OF THE DRAWING SCHOOL OF THE FRANKLIN INSTITUTE FOR THE SESSIONS 1890–91.

The year which closes this evening has been the most successful one in the history of the school, both in the number of pupils and in the system pursued in the teaching. In the department of mechanical drawing particularly the course is so well graded and so complete, without containing anything useless or merely time-consuming, that the results have in many cases been surprising. All theories have been made to conform to the best known practice, and the two have been carried along in harmony. This has been possible because the instructors have not merely received the conventional school-training in drawing but have all had practical experience.

The use of the new text books has been of very great service in ensuring systematic methods, and in economizing the time of both pupils and instructors. This has shown so plainly in the mechanical classes that an effort will be made to have one ready for the architectural class before the next term begins.

Architectural drawing, in many respects, is not in so advanced a condition as engineering drawing, because the methods of architects have been selected and adapted to suit the average contractor in the building trades, whose estimates on the cost of work are apt to be increased, as experience has shown, by the perfection of the drawings and the degree of their exactness. This may eventually be overcome when the trades schools, which are being established, have had time to produce their effect.

The free-hand department maintains its high standard, and its product is of a character to be understood and appreciated by the laity, and to speak for itself. Special attention has been paid to pen-and-ink work with very gratifying success.

The instruction throughout the school has been individual, class instruction with black-board demonstrations having proved a failure, owing to the great difference in the qualifications of the pupils. When each individual is treated on his own merits, he can be strengthened where he is weak and is not held back where he is strong.

The re-arrangement of the class-rooms, which was effected at the beginning of the preceding year, for the purpose of accommodating the entire school on two evenings each week, has again been insufficient this year, and the school has had to be divided into two sets on different evenings. As is the case with all the other departments of the Institute, the drawing school is cramped for want of room and accommodations.

The success of the school is largely due to the ability of the instructors, Messrs. Clement Remington, George W. Irons, George S. Cullen, John F. Rowland, Lucien E. Picolet and H. Allen Higgins.

The following pupils are entitled to Honorable Mention:

In the Junior Classes.

Joseph H. Moore,
August Mylander,
William T. Westbrook, Jr.,
Benjamin H. Barton,

Herman Klinefelter,
Frank Hopkinson,
Charles H. E. Price,
Frederick R. Baker.

In the Intermediate Classes.

L. C. Dilks,
Walter Hensel,
Frank J. Crowne,
William E. Arnold,
William Weinrich,
C. Q. Hewitt,
Thomas Worn,
William S. Cooper,
John Rigling,
David W. Perry,
Henry R. Johnson,
A. S. Mackenzie,
George T. Coleman,
Edgar R. Eavenson,
Julius Block,
J. Lyndall Hughes.

In the Senior Mechanical Classes.

Dinwiddie J. Luckett,
William L. Miggett,
William Hayes,
Eustace Rimmer,
John Bayne,
W. H. Sheahan,
R. H. Trimble,
M. A. Sykes,
James Mawson,
Charles Kolb.

In the Architectural Class.

A. Albertson,
Frank L. Borie,
Benjamin G. Barnett,
Henry Kerr,
Richard T. Given,
Wesley L. Blithe.

In the Free-Hand Class.

Robert F. Schleicher,
Walter L. Allen,
G. A. Davison,
August Schimpf,
J. G. Sullivan,
Thomas Winterbottom.

The following pupils are awarded scholarships from the B. H. Bartol Fund, entitling them to tickets for the next term, beginning September 22, 1891:

W. H. Sheahan,
L. C. Dilks,
Walter Hensel,
William T. Westbrook, Jr.,
Robert F. Schleicher,
William L. Miggett,
John Rigling,
Joseph H. Moore,
August Mylander,
August Schimpf.

The following pupils, having attended four terms and completed satisfactory a full course, are awarded certificates to that effect:

Benjamin G. Barnett,
Wesley L. Blithe,
Philip Bradley,
James J. Burke,
Frank A. Butz,
Joseph C. Douglass,
William H. George,
John Bayne,
Frank L. Borie,
Charles Burk,
Arthur H. Burling,
Michael Carlin,
Charles Friend,
Charles L. Henry,